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UNDER THE GENERAL EDITORSHIP OF
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COLUMBIA UNIVERSITY

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THE PSYCHOLOGY OF LEARNING
by E. R. Guthrie

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THE PSYCHOLOGY OF LEARNING

BY

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CONTENTS

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PREFACE

In so far as a writer can account for his own opinions and prejudices this essay on learning is a natural development of the views expressed in a book, *General Psychology in Terms of Behavior*, which was written in collaboration with Stevenson Smith and published some thirteen years ago. Although Smith has had no direct part in the writing of the present book a history of some twenty years of argument with him over the lunch table makes the determination of responsibility for much of what is here offered quite impossible.

Like the earlier book, this essay is written in terms of behavior. My prejudice in favor of a behavioristic attack on the problems of psychology dates from the Christmas vacation of 1910 in which I heard Professor E. A. Singer deliver an address at Princeton on "Mind as an Observable Object." That address remains the most stirring event of my academic life. Singer's contention that a relatively objective method could be applied to the scientific treatment of mind revived an interest in psychology that was waning as the result of an excursion into the field of psychophysics.

The notions described in the following pages are not new. Most of them have been familiar for a generation and some of them for centuries. Associative learning has

been recognized in some form or other by every writer on psychology since Aristotle. What has been here attempted is an exploration of the field of learning to discover the nature of the phenomenon of association and the limits of its use in the explanation of learning.

Without a reading by Professor E. A. Esper and editorial comment full of "insight" by Professor Gardner Murphy many more faults would have been included than will now be made public.

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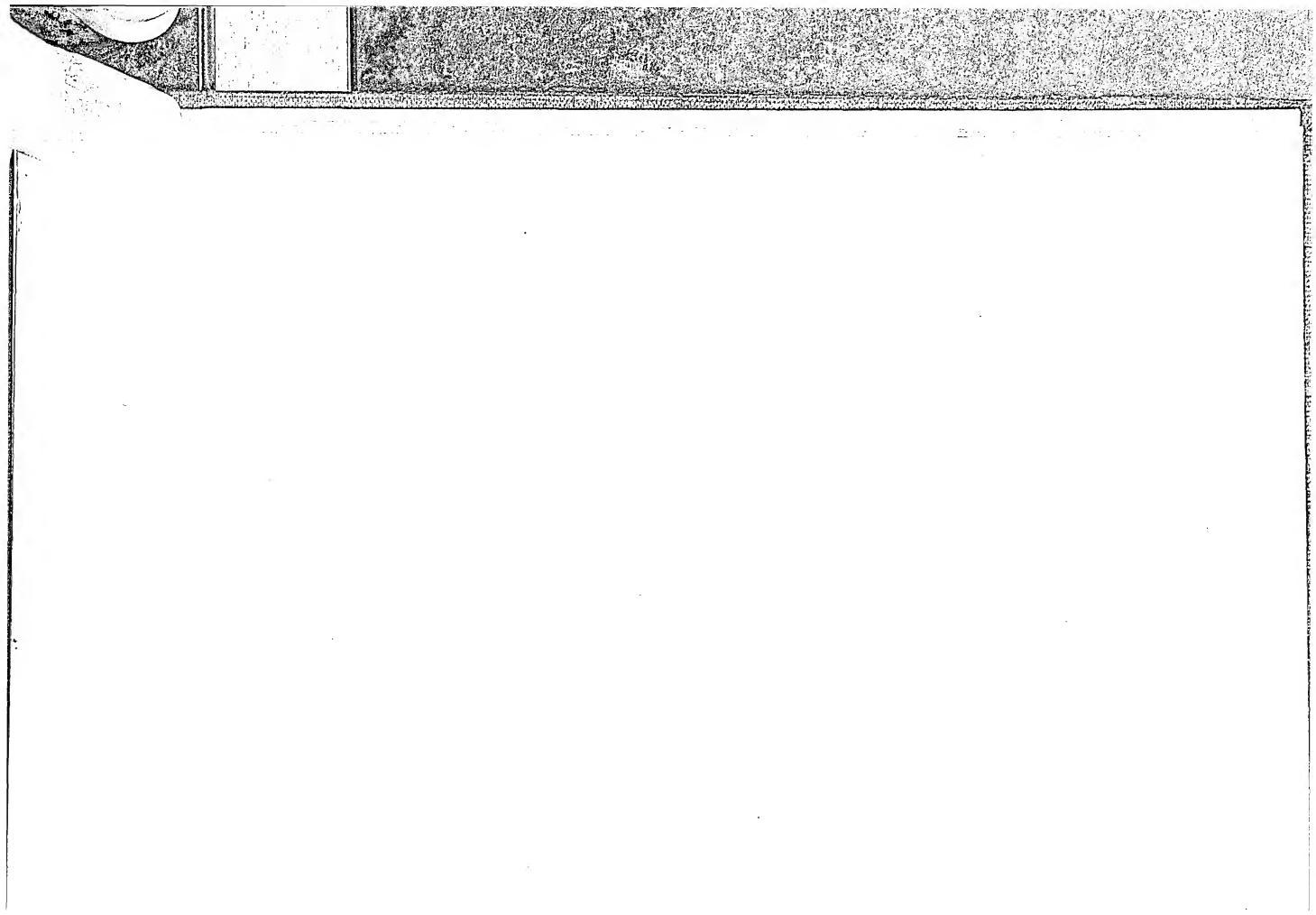
E. R. GUTHRIE

Seattle, July 27th, 1934

The Psychology of Learning



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Chapter I

INTRODUCTION

PSYCHOLOGISTS nowhere depart farther from common sense than in their notion of the nature of minds. The man on the street acknowledges that minds are rather mysterious, but he is definitely sure that a mind is something that you either have or you haven't. Bricks haven't. He has, and knows that he has. Dogs also have minds. Angle-worms? Here he becomes a little doubtful. Angle-worms seem very definitely to resent indignities in a way that a brick does not. And their daily round of activity seems to have something of a plan and purpose behind it. A brick reacts to a kick by moving over, but there is a distinctly passive and helpless air about the brick's behavior. And the brick is so dependable; it does regularly the expected thing when it is disturbed. If it behaves differently on a second occasion we can find the occasion for the difference. It is a different brick. If it did not fall apart at one blow of the hammer, but now does fall apart when struck the second time with the same force we take for granted that the first blow cracked it. If a bridge timber gives way after years of use under a load that it has held up to the present moment, we assume that it is a different timber; dry rot has invaded it or past stresses have cracked it.

But do angle-worms have minds? Are growth and reproduction and defensive reaction enough to qualify the worm for that distinction? Plants also grow and multiply and defend themselves not only by their structures but in many cases by movement. Common sense is inclined to deny that plants have minds, for this is an opinion shared only by a very few detached sentimentalists.

What is it then that plants lack that is to be found in creatures which common sense endows with minds? Strangely enough, common sense will be found to offer a very good answer to this question. Growth and reproduction and defense reactions are life, but they are not mind. Mind is these and something more; it is growth and reproduction and reactions serving these ends plus something that common sense might call profiting by experience. The answer to the question of the angle-worm's status will be determined by the answer to the question: Does the worm respond always the same way to the same combination of circumstances, or does the worm alter its response as a result of its past experience?

Of course, a full worm and an empty worm respond differently to the world. This is not what is meant by profiting by experience. The altered behavior here is like the altered behavior of the bridge timber; it can be referred to differences in the present circumstances. The difference can be examined and the response understood on the basis of what is now the condition of the worm.

Now it happens that a psychologist has established

that worms can profit by experience. Yerkes placed earth-worms in a T-shaped maze and found that if a slight electric shock was always administered when the worm turned, say, to the left, ultimately the worm was in some fashion changed so that it more or less regularly turned to the right and avoided the shock.

The difference between a worm that has received this Harvard laboratory training and a worm that has not is a difference that can not be discovered by examining the worm. The training leaves no observable changes. All psychologists believe that differences accounting for the altered behavior exist in the worm's "brain," but it is doubtful whether we shall ever be able to examine these brain differences either during the life time of the organism or at a post-mortem. Such traits as an acquired liking for mince pie, or skill at chess, or an ambition to travel, which are all modifications of behavior like the right-turning habit of the earth-worm, are not by any present technique possible of demonstration at an autopsy. We may speculate concerning the nature of the brain changes that lie behind these habits, but that speculation will throw no light on the nature of the habits.

We shall call these changes in behavior which follow behavior learning. The ability to learn, that is, to respond differently to a situation because of past response to the situation, is what distinguishes those living creatures which common sense endows with minds. This is the practical descriptive use of the term "mind." Another use, the theological or mythological notion of mind as a substance, as a mysterious hidden cause of ac-

tion, we may dismiss at once. Our interest is scientific, and we are dealing only with observable features of the world about us. Mind must be for us a mode of behavior, namely, that behavior which changes with use or practice, behavior, in other words, which exhibits learning.

Learning, as so defined, does not include all changes in behavior tendencies. Fatigue is, for instance, a change in behavior, but it is referred not to action primarily as its occasion but to altered chemical states in muscle and in the blood stream. There are other changes in behavior tendencies which might be included in the term "learning" if we were so inclined. When continuous pressure is exerted on a touch receptor, Adrian has recorded instances in which the receptor responds only for a brief period. Impulses from the sense organ are demonstrable in the sensory nerve only for a few seconds, though the pressure on the sense organ is continued. This is a change in behavior tendency, since the organism will no longer respond, though the stimulus continues; but this is a very temporary change and from it there is a quick recovery. Humphrey, in his interesting recent treatise on learning (1933), advocates using that term to include such temporary changes in behavior as fatigue and sense organ adaptation. I prefer to reserve the word "learning" for the more lasting effects of practice. Later in this book reasons will be offered for the belief that learning is normally permanent except in so far as it is in its turn altered by new learning. Transient changes like fatigue and sense organ adaptation disappear after brief intervals through physiological processes.

The definition of learning in terms of the lasting effects of practice is not a definition on which all psychologists would agree. There are other common meanings of the word. Common sense and many psychologists have used "learning" to refer only to those changes which contribute to the accomplishment of some end or purpose. In this sense learning always means learning to do something, learning to write, learning to skate, learning, in short, which results in an ability or a skill or a capacity for some achievement. This identification of learning with the attainment of a good result is all very well for common sense, but for a scientific understanding of human behavior it will not serve. And the reason that it will not serve is that in the same manner and in the same ways that human beings acquire skills and capacities they also acquire faults and awkwardnesses and even lose capacities which they once possessed. Since virtues and skills are acquired in the same way that faults and awkwardnesses are acquired, it seems unreasonable to limit the meaning of the word learning to achievement. It is true that the changes referred to as "learning" do generally turn out to be beneficial, that they are in the long run adaptive, and this must be looked into; but we have deserted the methods of empirical science if we assume that all learning is good, that every action has its goal. There are psychologists who believe this, not only of their own actions but of all the actions of all animals. The hen lays her egg, not because it has reached an embarrassing size, but because the species must be preserved. She is

aiming at motherhood and carrying a torch for her species, and not just laying an egg.

In this present account it will not be assumed that all learning is a progress toward betterment. Learning will be understood as change rather than as improvement. Our task is to understand the circumstances under which learning takes place and the nature of the changes that it involves. Our method should be to survey the experimental work on learning and to review what is common knowledge of learning and to try to discover any generalizations that can be made from our survey. Can we find any rule or uniformity in the phenomena? Can we describe any circumstances which regularly have a certain kind of outcome? Does the animal which has had one kind of history afterward tend to do certain things? *Under what circumstances do the specific changes in behavior we call learning take place?*

If we find such rules, they will not only be an adequate theory of learning; they will also direct the practical advice we give to persons who are guiding learning. Our rules, to be good theory, must be based on observation and verified by observation. This is one requirement. Another is that they shall be as concise and clear as we can make them. Our antecedent circumstances must be so clearly described that other persons can from our description recognize instances of what we describe; and this must also be true of our alleged consequences. If our descriptions are vague or ambiguous our rules can not be verified; nor can they be used for the anticipation and control of behavior. These are the

important requirements for psychological theory, and we may note that these would be the most important requirements for practical advice.

If we can find, on examining our common knowledge and the experimental work on learning, that certain describable, observable, and recognizable antecedent conditions enable us to predict certain describable, observable, and recognizable changes in behavior, we shall have discovered laws of learning. These laws will constitute our explanations of learning, for all scientific explanations are nothing more than generalized laws or rules which cover the event needing explanation.

The search for these laws has certain inherent difficulties. The first of these difficulties has to do with language. Putting events into words is never entirely satisfactory. Here are pupils in a class-room. The teacher gives them a spoken direction. How are we to describe this as a stimulus to the pupils? No two pupils see the teacher from the same angle or hear her voice from the same distance. No two pupils move their eyes alike, consequently no two have the same retinal activity. The optical properties of different eyes differ. What the pupils hear and what they see depends on the form of their present attention and on their previous experiences, which are various. We are forced to speak of the voice of the teacher as a stimulus, but we are forced to speak vaguely. We can never be sure that the stimulus of the voice does not affect different pupils in essentially different ways.

Reactions are just as hard to describe and name. Popu-

lar names for most acts are names for end results which may be accomplished in an indefinite variety of ways. Accepting an invitation, going to market, attending a dinner, playing a tune, catching a fish, all name acts, but the acts they name are left indefinite. It is only because the acts we name have a rough practical equivalence that we are able to undertake their prediction. Under these handicaps our forecasts of action are bound to be inaccurate and we must be resigned to finding exceptions to all our rules. We shall be dealing with tendencies and not with certainties.

A second difficulty in codifying the laws of learning has been introduced by psychologists themselves. Experimenters in the field of learning have failed to make clear to the public or to themselves that two fundamentally different kinds of research have been in progress. Some psychologists and physiologists have been interested in the prediction of movement or glandular secretion without any reference to the utility of the movement or to its consequences. Pavlov, for instance, is interested in discovering the circumstances under which a dog will secrete saliva in response to stimuli which were previously ineffective. The ability to secrete saliva at the sound of a bell or at a touch on the flank is of no use to the dog after the experiment, though similar conditioning in natural conditions may be useful. Pavlov is interested in the phenomenon of conditioning, not in its utility. Tolman, on the other hand, records the fact that the rat in the maze reaches food, its goal, and is not concerned about the movements by which the goal is

reached. The psychologist whose interest is in the goal-reaching capacities of animals will make goal-attainment the entry in the record, and not the means used, which may be varied. One experimenter like Pavlov is interested in the process; other experimenters are interested in the results. It is to be expected that the two types of workers will discover very different laws of learning. The conditions under which goals are reached are not at all the conditions under which habits are stereotyped. A day-old chick will peck at grains and capture a certain percentage of them. Its percentage of success increases rapidly with practice, but if the chick formed a stereotyped habit of pecking in one direction and with a fixed reach it would retrieve very few grains.

The differences in the results announced by different psychologists arise from differences in the modes of behavior that they are intent on predicting. Their findings are not contradictory. Empirical studies, if they are honest, can not be contradictory. Pavlov's question, under what circumstances can a stimulus not previously the occasion of a response become a substitute cue for that response will have one answer, while the question, under what circumstances will a man or a dog acquire a certain skill or ability will have another. It is, of course, this second question that has the more practical interest. To it the later chapters of this book will be devoted.

Two sources of difficulty in the formulation of laws of learning have been mentioned, the difficulty of fitting language to the description of the confused and intricate flow of behavior, and the failure of experimenters to

record the same features of behavior. There is a third difficulty. We can not record or control all the conditions under which our experiments are made, or record all the details of any sample of behavior. The physicist is less embarrassed by this obstacle. He does not concern himself about the recent night-life or the childhood experiences of the bit of metal whose density he is determining, whereas such items of history may lead to very bizarre results in the psychological laboratory. Even with this advantage we find that the physicist tends to flee from reality into a dream world of "ideal" gases and liquids, because these are the only ones that will obey the laws of physics. Boyle's law that in a gas with temperature held constant the product of pressure and volume is a constant is not true of any real gas. And when the physicist turns engineer and undertakes to predict the behavior of actual things in a real world, he protects himself with safety factors of 600 to 1000 per cent to allow for any shortcomings in his predictions.

The psychologist must resign himself to the fact that no psychological event is ever really repeated. The second repetition of a stimulus is only roughly and for practical purposes equivalent to the first; his laboratory subject is only substantially or approximately the same person who sat in the chair the day before. Since that time he has slept, eaten a little, learned a little, and this will alter his response no matter what precautions have been taken to have conditions the same. No two responses are alike. Two trips through a maze, two conditioned salivary reflexes may be substantially the same,

but they are always the same with a difference. As a result of this indescribable complexity of events we are limited to prediction with a high degree of error. We can attach only probabilities to expected events and the probabilities may be very slight. Our comfort lies in the fact that even slight probabilities in the expectation of human conduct may be better than complete ignorance.

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Chapter II

PSYCHOLOGICAL EXPLANATIONS

PROFESSIONAL explainers regularly forget that there are two parties involved in an explanation, the one who offers the explanation and the one at whom it is directed. An explanation that does not make clear the event to the person seeking information is no explanation at all. It is not the explainer who must be satisfied but the listener.

There are now current several kinds of psychological explanations of human and animal behavior which all satisfy their inventors, but the rest of us are left in some confusion. There are no true explanations and false explanations, but only successful and unsuccessful explanations, acceptable explanations (acceptable to us, the hearers) and unacceptable explanations. If we believe in fairies, events can be explained to us as the actions of fairies. The explanation will be unscientific only because the scientific tradition is not acquainted with fairies and scientific men have discovered that they can get on well enough without recourse to spirit agents of just this sort.

An explanation of any event consists in stating the rule or generalization of which the event is an instance. Scientific explanations have placed upon them at the start a severe restriction. They are directed at the prediction

and control of natural events, not at arousing awe, or giving comfort, or forestalling questions. Scientific explanations can not invoke the will of God because to explain a happening as an instance of the will of God may lead the questioner to be resigned but it does not lead him to foresee what will happen in such cases. Common sense uses many such too facile explanations in terms of the action of agents, and this is quite legitimate under many circumstances, but knowing that an event is the act of an agent does not give the rule of the act unless we know the ways of the particular agent, and science is a public and impersonal affair, not interested in particular agents. "Your chair," the housewife explains to her inquiring husband, "I put there because I had grown tired of seeing it where it was." This is an adequate explanation for some purposes. The husband knows something of what to expect when his wife is taken with this form of esthetic fatigue. But this is not a scientific explanation because science can not be burdened with a catalogue of the ways of particular human agents.

Scientific explanations all have much the same form. They state the rule of which the event in question is an instance. A state of affairs which we may call "A" (for "antecedent") is followed in a certain proportion (say x per cent) of the cases observed by another state of affairs "C" (for "consequent"). If the law has to do with the degree or quantity of some state instead of merely with its presence or absence it would read: State "C"

is a certain mathematical function of state "A," or $C = \Phi(A)$, with an observed error of estimate, "z."

In such a law both the antecedent and the consequent may be anything whatever, provided that it is observable, describable, and recognizable. The consequent, "C," will be something which we are interested in anticipating or predicting; the antecedent, "A," will be anything that can conveniently serve as a warning of "C." The rule itself, if it is to qualify as scientific, must be the result of observation of cases of "A" and of "C" and it should be verified by further observation after it has been formulated.

All of this will undoubtedly prove rather discouraging to a reader who is interested in learning and not in a general discussion of the nature of science, but it has much to do with an understanding of the apparent differences between psychologists who are engaged in the study of learning. The differences between psychological schools lie in what they choose to employ for the "A" and "C" of our formula. Many psychologists are now engaged in finding what behavior may be expected from knowing that an individual is a member of a certain species. A great deal of such information has been collected. If our antecedent knowledge is that this individual is a rat we know something of the kinds of things we may expect of it. What can be expected of rats in a maze has been the topic of an enormous amount of recent investigation. This knowledge is usually stated in the form of instincts, or lists of results that rats may be expected to attain. Some psychologists, such as Wheeler

(1929) and Humphrey (1933), aim to describe what may be expected of an individual knowing only that it is an organism.

Living organisms, according to Wheeler, obey the law of least action; this is, according to him, the fundamental law of living as well as of non-living systems. In physics this law would state that the movement of a body from one position to another in a given time interval is always in such a path that the difference between its kinetic energy and its potential energy will be a minimum. This holds for conservative systems in which the sum of kinetic and potential energy is a constant.

Let us examine this law in use. Hungry rats placed daily in a maze at the end of which there is food will eventually eliminate waste motions and find the shortest path to the reward. According to the "organismic hypothesis" of Wheeler this is merely an instance of the general law that the path of any particle in a system will be the path involving least effort. This covers all the phenomena of learning and habit formation. It is interpreted to mean that all behavior of organisms consists in progress toward a goal, and we may expect that this progress will *eventually* take the path of least effort from the start to the goal. Hence we know in advance what will be the results of learning, though we do not know just how these results will be attained.

There is an immediate objection to this notion. The law of least action as it is used in physics does not use the word "eventually." Movement in a physical system is supposed to be conforming all the time to the law of

least action, and not to require, say, a week's notice. Living creatures obviously do not start out with the path of least effort. Their gradual approach to this path Wheeler calls "maturation" and this is acknowledged to require time.

This difficulty with the law of least effort has been surmounted by Humphrey in his *The Nature of Learning*. If we find it disturbing that living creatures require time to approximate achievement with least effort, all that we need do is to regard time as a fourth dimension and we discover that in the resulting space-time continuum the law is obeyed.

This is an exhilarating doctrine. To discover that the confused phenomena of learning, the laws of practice, the effects of punishment and reward, forgetting, association are all instances of the law of least action which applies to atoms and molecules as it does to gross human action is to find man a part of nature with a vengeance. My own exhilaration on reading this doctrine has been rather soon damped. I find to it a number of objections. Some of these will be described in following chapters. Here they can be only summarized.

In the first place, the physical law of least action holds only for conservative systems, that is, systems in which the sum of kinetic and potential energy is constant. Organisms are not closed systems in this sense and they are in constant interchange of energy with the environment. In the second place, the changes that are involved in moving from a start to a goal can not be described in terms of mass, length, and time. In these terms it is im-

possible to define such an end result as the attainment of skill in driving a car (of which the measure would be the number of accidents) or of proficiency in playing the piano. Unless such a definition could be given in terms of centimeters, grams, and seconds there would be no method for discovering whether or not a minimum expenditure of energy had been reached.

A third objection lies in the assumption that all organisms will move inevitably toward perfect execution of all practiced acts. It is true that Wheeler places a limitation on this. The act or the achievement must be within the animal's range of insight if the law is to hold. The law holds if we disregard the exceptions. This excuses all failures in advance. I have a personal conviction amounting to a strong prejudice on this issue. Having sojourned in this life almost a half century, I am aware that most of my movements are of very debatable usefulness and that most of my time is misspent. What few goals I have been aware of setting for myself have been in large proportion avoided, and this appears not at all a cause for regret. An *a-priori* natural law that makes the attainment of goals in minimum time and expense of energy a certainty, and admits the amount of confusion and conflict and error that prevail among my own goals, appears to me a little absurd.

My last objection to this form of the organismic hypothesis as a theory of learning is perhaps only the previous one in another form. If our explanations of learning, our rules governing all cases of learning all predict success with minimum effort we are bound to be un-

pleasantly surprised, because failure and waste are perhaps even more prevalent than is success. Even the tendency to self-preservation is always defeated in the end and minor goals have an even higher mortality rate if such a thing is possible.

In this form, therefore, there seems to be rather little use for the organismic hypothesis. In another form based on observation, empirical and not *a-priori* organismic views are compelling and legitimate. Observation of living organisms gives very little insight into the details of human behavior if it is confined to those traits which hold of all organisms. But if we patiently observe a species we can offer a great deal of important information concerning what to expect of members of that species. The work just published by Carpenter (1934) on the behavior and social relations of howling monkeys after two years observation of them in Panama makes us familiar with many of their ways, just as the work of Tolman and his associates has added to our information concerning what to expect of the white rat in a maze.

Information concerning what to expect of an individual if he is a member of our own species is also important. But there is a source of information still more important. *This source is the past history of the individual man.* Knowing a man's past history we can make specific statements concerning how he will behave in specific situations which would be quite impossible if all the information we had concerning him was that he was human. The best information we can gain concerning how a man will behave in a given set of circumstances

comes from the record of what he last did in these circumstances. Individual likes and dislikes, idiosyncrasies, response tendencies, the greater part of all that we can predict of the individual man is predicted in terms of the association of specific features of response with specific features of a situation. And the only form in which such information has been offered or can be offered is some form of association theory. Other theories may list the goals which men are often known to reach, but the anticipation of specific action must always depend for the most part on the use of stimuli as the weather signs of conduct and the past behavior in the presence of these stimuli as the indication of what is to come.

It will be noticed that such information will never violate the modes of behavior known to hold for all organisms, or known to hold for the human species. Membership in the species is a very persistent attribute of man.

The Gestalt psychologists have recently placed a great deal of emphasis on the assertion that behavior is always a response to a total situation. So far as I know this has never been disputed, and it is of great importance to bear it in mind. But any attempt to predict from the total situation is hopeless. "A," the antecedent term in our general formula for scientific laws, can not be a total situation because total situations are never repeated, and science can not deal with events that are unique. We can deal only with recurring aspects of events. The determining occasion for the World War could be found only by observing a series of comparable wars and find-

ing which of the antecedents were necessary and sufficient antecedents. This, of course, could not conceivably be done because of the uniqueness of the event. A great deal of human behavior is in this same category. For human actions that are unique, for the work of the creative artist and the original thinker there will never be any adequate scientific treatment because the creation and the thought occur once for all, and there will never be an opportunity to find what details of the artist's training, of his difficulties, of his love affairs, of his chance human contacts were the determining conditions for his work of art.

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Chapter III

THE CONDITIONED RESPONSE

In a Pacific Coast city recently a number of dogs succumbed to strychnine poisoning. Poisoned chunks of beef were found in the neighborhood. Several owners of good dogs undertook to train their animals not to indulge in stray tidbits by scattering about many pieces of beef to which were fastened small mouse-traps of the familiar spring variety. For the time, at least, they had a quick success. The dogs developed a very supercilious attitude toward stray meat.

The mother of a ten-year-old girl complained to a psychologist that for two years her daughter had annoyed her by a habit of tossing coat and hat on the floor as she entered the house. On a hundred occasions the mother had insisted that the girl pick up the clothing and hang it in its place. These wild ways were changed only after the mother, on advice, began to insist not that the girl pick up the fallen garments from the floor but that she put them on, return to the street, and re-enter the house, this time removing the coat and hanging it properly.

A mature student, a woman, has become so indignant over the noise of the radio in the next apartment that

she is unable to study in her own quarters. On the advice of an acquaintance she finds that mystery stories compete successfully with the radio for a week, and that at the end of the week study is again possible in spite of the noise.

A talkative three-year-old is at the dinner table. Her father has come from his business tired and angry. In answer to her request for something he turns suddenly to her and shouts: "Say: Please." The rest of the family joins his demand, but the child becomes excited and for a period of some eleven months has not spoken in the presence of her family.

All of these are instances of the modification of behavior following some experience. The first three are cases in which some person showed a rough knowledge of what to expect as the result of interference with behavior. They are examples of control. The hysterical mutism of the little girl illustrates the lack of such knowledge, and the effect of that lack. Explanations of learning put such knowledge into words, and the real test of any explanation is this: Does it enable us to anticipate behavior and adjust ourselves to it in advance, and does it enable us to interfere with behavior and be prepared for the results of our interference?

The law of least action, that the difference between the kinetic and potential energies for any actual motion in a given time interval from one configuration of a system to another shall be a minimum offers cold comfort to dog-trainers or to parents. And yet any college freshman to whom the cases above are described can give an

approximation of the result to be expected. Obviously some form of association is involved. This also the college freshman can state, though his statement may not be as clear as we might ask.

Associative learning has been recognized since there has been any writing on the subject of human nature. Popular sayings have made knowledge of it available for generations. "Once bitten, twice shy." "The burnt child dreads the fire." Aristotle described its nature and used it to explain recall. The oldest and most persistent observation concerning associative learning is the law of association by contiguity in time, a rule that has been noticed by practically all psychologists since Aristotle.

The psychologists of modern times have usually treated associative learning as though it were concerned with the association of ideas, though their illustrations show that they considered action included in its scope. In recent years emphasis on movement as the observable denotation of "thinking" has led to a change in its wording. It is now called the conditioned response by many psychologists, but it is fundamentally identical in meaning and use with the principle of association.

The principle has been stated in many different ways. Bishop Berkeley speaks of "an habitual and customary connection" by which one idea is the occasion for another. For Hume there was a "gentle force" by which "one idea naturally introduces another" after they have occurred together. James Mill says: "Our ideas spring up, or exist, in the order in which the sensations existed, of which they are the copies. This is the general law of

the 'Association of Ideas,' by which term, let it be remembered, nothing is here meant to be expressed but the order of occurrence." For him this association could be successive or synchronous. The strength of an association could be measured by its permanence, its certainty, or by its "facility." The conditions determining the strength of an association were frequency and vividness.

By the time of James Mill the doctrine of association was so much involved with the analysis of consciousness, which was the proper and peculiar domain of psychology, that action and performance were lost sight of. According to Bain: "Actions, Sensations, and States of Feeling, occurring together or in close succession, tend to grow together, or cohere, in such a way that, when any one of them is afterwards presented to the mind, the others are apt to be brought up in idea." Hartley's statement of the law was: "Any sensations A, B, C, etc., by being associated with one another a sufficient number of times, get such a power over the corresponding Ideas, a, b, c, etc., that any one of the sensations A, when impressed alone, shall be able to excite in the Mind b, c, etc., the ideas of the rest."

William James states it thus: "Objects once experienced together tend to become associated in the imagination, so that when any one of them is thought of, the others are likely to be thought of also, in the same order of sequence or coexistence as before." He also is referring to ideas rather than to action, but he has prepared for his law of association by the remark that, "The laws of motor habit in the lower centers of the nervous

system are disputed by no one. A series of movements repeated in a certain order tend to unroll themselves with peculiar ease in that order for ever afterward. Number one awakens number two, and that awakens number three, and so on, till the last is produced. A habit of this kind once become inveterate may go on automatically. And so it is with the objects with which our thinking is concerned."

In spite of this introduction of the association of ideas by the "laws of motor habit" which are "disputed by no one" and in spite of his incidental remark (*Principles*, Vol. 1, page 124) in his famous chapter on habit that: "It is not in the moment of their forming, but in the moment of their producing motor effects, that resolves and aspirations communicate the new 'act' to the brain," James failed to see the application of the notion of the association of ideas to movement and to action. In his short chapter dealing with habit he is content to say that "our nervous system grows to the modes in which it has been exercised" and to point out the results that "habit simplifies the movements required to achieve a given result, makes them more accurate and diminishes fatigue." and that "habit diminishes the conscious attention with which our acts are performed." (Vol. 1, page 112). He does point out that "In action grown habitual, what instigates each new muscular contraction to take place in its appointed order is not a thought or a perception, but the *sensation occasioned by the muscular contraction just finished.*" This implies the rule of the conditioned response, but does not expressly recognize it.

The form of the principle which is in my opinion the most useful is this: *A combination of stimuli which has accompanied a movement will on its recurrence tend to be followed by that movement.*

Although this is a short and simple statement, it will not be clear without a considerable amount of elaboration. The word "tend" is used because behavior is at any time subject to a great variety of conditions. Conflicting "tendencies" or incompatible "tendencies" are always present. The outcome of any one stimulus or stimulus pattern can not be predicted with certainty because there are other stimulus patterns present. We may express this by saying that the final behavior is caused by the total situation, but we may not, in making this statement, flatter ourselves that we have done more than to offer an excuse for a failure to predict. No one has and no one ever will record any stimulus-situation in its totality, or observe any total situation, so that to speak of it as a "cause" or even as the occasion of a bit of behavior is misleading.

The principle of conditioning thus stated with its word "tend" merely asserts that on the recurrence of a stimulus pattern we can expect the former response, but with what certainty it does not state. The value of the probability can only be assigned as the result of observations of a number of like cases. If in one hundred instances of the recurrence of the stimulus pattern we find forty-one cases where the movement in which we were interested recurs, we may write this down as a law and add it to our psychological information. *The principle*

of conditioning is not an explanation of any instance of learning. It is merely a blank form which explanations of instances of learning may take. It expresses only the conviction that we may profitably look for the signs of a movement among those stimuli which are now present and which were once before present with the movement.

So much for the word "tend." The phrase "combination of stimuli" also requires elaboration. "Stimulus" is a Latin word. The English for it is "goad." A goad is not the entire cause of the movement of the goaded animal, but only its occasion. A goad is ineffective on a stone or on an exhausted beast. Stimuli, likewise, are not the entire causes of movement, but only certain convenient signs of the movement to come. Movements do not always follow stimuli, but it is, so far as the psychologist or physiologist knows, true that movements of the striped muscles which are employed in moving the body or in thinking are always preceded by stimuli. Stimuli are the necessary but not the sufficient conditions of movement. The muscles of the viscera, the smooth muscles, may contract and relax in response to chemical changes in their medium, or to internal rhythms. But movements of the striped muscles are generally believed to follow only when motor nerves are excited, and again, so far as we know, this excitement of motor nerves is in all normal behavior preceded by sensory nerve impulses, which are in their turn normally preceded by those changes at the sense organ which we call stimuli.

Having used the word many times we may now offer a belated definition: Stimuli are changes in the world

order to which sense organs or receptors respond by exciting impulses in sensory nerves. At the risk of going somewhat astray from the topic of conditioning something further must be said concerning the nature of stimuli. *The views of learning presented in this book are not dependent on any special theories of the physiology of the nervous system.* For the purpose of understanding learning, stimuli could be defined as changes in the physical world that occasion observable reaction on the part of an animal. But I am nevertheless strongly inclined to view the rôle of the nervous system as a system of pathways connecting sense organs with muscles and glands, pathways over which nerve impulses travel. I am also strongly inclined to believe that the occasion of every thought as well as of every act is to be found in stimuli acting on receptors. Thought is born in action, paradoxical as that may seem to those who notice how often action depends on thought; but it is possible to doubt, as I do, that we can report any thoughts which are not embodied in movement. Activity in the nervous system is, fortunately for the owners of nervous systems, normally dependent on a beginning in the sense organs by which we are in touch with the world and its changes. A train of thought, to be sustained, must somewhere effectively stimulate sense organs. The sense organs stimulated are those which are sensitive to our own movements, the proprioceptors, scattered throughout the skeletal muscles and at tendons and joints.

Stimuli can in a great many cases be observed and recorded. This is the reason that they serve so well as the

warning of oncoming action. It is very rarely that we can observe the direct impact of change on the sense organ. What we can observe is the source of light, not the beam that enters our subject's pupil; or we hear or record the sound wave that must affect his ear, the same wave but on a different section of the wave front; we can see and record movements and changes in posture, but the sense organs that these movements affect are hidden from direct observation. Movement, light, sound, pressure, skin contact, the contact of various substances with the olfactory membrane and with the taste papillae, these are what we mean by stimuli. The world may alter us by other means, by wounds, disease, by the nature of the food we eat, but it is only to changes that affect sense organs that we respond in the manner that is called behavior.

But something more must be taken into account in understanding the effects of stimuli. Through stimuli we are continuously in touch with the world and our own bodies. The resultant behavior is a response to this total stimulation, a continuous compromise, a continuous integration. It would be hopeless for either psychologist or the man who has no interest in psychology to try to keep an eye on this totality of stimulation. The best that either can do is to select certain outstanding and conspicuous parts of the total stimulus situation and keep some record of their consequences. Among these particular stimuli that are selected for use as warnings of the act to come, we find that combinations of stimuli may have an importance that the component elements

would lack. It is possible for stimulus combinations to act as signals of a response when parts of the combination would have no effect.

To return to our principle, something must be said of the nature of the response. The principle was stated thus: A combination of stimuli which has accompanied a movement will, on its recurrence, tend to be followed by that movement. Movements, like stimuli, are complicated. We can not possibly follow or record the total response. All that we can do is to notice some feature of it, and attempt to forecast this. But the movement in response to stimuli is not a detached movement and it is affected by many stimuli which are not on the record. It will be affected by other movements and by our posture, as well as by those stimuli which remain outside our calculations. In other words, we will never get exactly the movement we were looking for. I may pick up a hot object and then hastily drop it. I have been looking at it as I picked it up and as I let it go. If we took the principle of conditioning literally, I might be expected at the next glimpse of the object to go through the motions of dropping it although it is not in my hand. As a matter of fact something like this often occurs. It is, however, more probable that the next sight of it will cause some movement of the muscles used in letting go but that this movement will be much disguised because it is in another context; the hand is not in the same position grasping the object. It may now be in my pocket, or engaged in holding a pipe. The visual pattern of the hot object is not the only stimulus acting; the pipe is in

contact with the skin and the muscles used in grasping are maintaining contraction through circular nervous pathways from their own sense organs. These other sources may insure that grasping will be maintained, and the conditioned response may be present only in the form of a minute relaxation of the muscles.

I am taking a walk and at a certain point in the walk I meet a friend and stop to chat with him. When I repeat the walk the context of stimuli in which I met the friend may recur, but in the absence of the friend and in the strong tendency for an activity like walking to maintain itself through learned, associated sequences of movement I do not on this occasion stop and converse with the empty air. All that is evident of the conditioned response may be some remnant that constitutes thinking of the friend. This may include some traces of the greeting. We say that we are reminded of the friend at that point.

The conditioned response will thus never be the exact duplicate of the original behavior; but it will on many occasions be enough like the original behavior to make its prediction important.

Razran (1930) and Rexroad (1932) have each suggested an extension to the principle of conditioning which would recognize the fact that the dominating response has an advantage over a dominated response. Rexroad would formulate this as follows: "A stimulus gains effectiveness for a given response when the stimulus is followed by that response as a dominant response."

To illustrate: If a dog is offered food and at the same

time struck a blow, his response is a compromise between eating the food and avoiding the blow. Neither whole-hearted eating nor complete avoidance will occur. If this situation is repeated, there is no rule by which we can tell in advance whether the final result will be that he will slink away on seeing that particular food, or that he will eventually secrete saliva on receiving the blow, making the blow a mere appetizer. This is not an absurd result because Pavlov's laboratory has found that blows, pin pricks, or burns could be made signals for eating and saliva flow, and the traces of the original protective movements practically disappear. Rexroad's principle would hold that the dominant response, the response that was most in evidence, would eventually win.

I believe this extension of the principle of conditioning as it has been stated in this chapter is of very doubtful use. The principle of conditioning states that the movement that occurs will be the movement conditioned. The only test of dominance is the occurrence already provided for in the principle of conditioning. In the illustration just offered, the result of the first experience will undoubtedly be that both food and blow tend to call out the compromise. This may last indefinitely, and the dog for a long period may take that particular food under those particular circumstances with little enthusiasm, even though the blow is not repeated. For one response or the other to occur there must be further training in which the "dominant" response is facilitated, or in which the dominated response is lessened or inhibited. Dominance is a function of the relative in-

tensity of stimuli and of facilitation and inhibition from the remainder of the situation. There can, consequently, be no general rules for dominance in advance of the event except experience which indicates that one response rather than the other *will occur*, and if it occurs the principle of conditioning indicates that it will be the response conditioned.

One more feature of the principle of conditioning as here offered may be noticed. It is confined to simultaneous association. The stimuli present *as the response occurs* are the future cues for the response. How this may be reconciled with the experimental findings that responses are associated with stimuli that precede or follow them, sometimes with an appreciable interval between, will be described in the chapter on Time Factors in Conditioning.

The most extensive laboratory investigations of the conditioned response have been made in Russia, following the pioneer work of Pavlov, whose interest in the conditioned salivary reflex dates from the 'nineties. Laboratory experiments have brought out a large number of characteristics of the conditioned salivary reflex and of conditioned motor reflexes. Razran (1933) has made excellent summaries of the results of work on the conditioned reflex, including much material not before available in English. We may list separately a few of the best established generalizations, most of them suggested by Razran:

1. The simple conditioned reflex is most readily established when the substitute stimulus is given shortly

before the original stimulus (the stimulus used in the laboratory to elicit the reflex to begin with).

2. Simultaneous and backward conditioning are possible, but less effective than forward conditioning.

3. A substitute stimulus which has, to use Pavlov's language, been conditioned to a response loses this effect when it is repeatedly given without reinforcement from the original stimulus. This Pavlov calls *temporary extinction.*

4. This loss of effect can proceed farther than the zero point, and the "extinguished" stimulus will have an inhibiting effect on the response.

5. Temporary extinction may disappear after a period of rest, or after disturbance from a new irrelevant stimulus.

6. When extinction is carried out repeatedly, recovery is progressively diminished, until it fails to take place.

7. Having conditioned S_1 to a response, S_2 may be conditioned by being presented with S_1, S_3 by being presented with S_2, and so on. In my opinion this is not an important generalization, since the essential condition of associative learning is the association of the new stimulus *with the response, not with the original stimulus.*

8. A response involving wide-spread bodily action is more readily conditioned than a response confined to a few local effectors.

9. There seems to be evidence that among children conditioned responses are formed more readily as age advances, and more readily in the more intelligent.

10. One attempt to form a conditioned response dur-

ing sleep has been a failure. Conditioning failed in another experiment by Harlow and Stagner (1933) when skeletal muscles were paralyzed by curare, though smooth muscle reflexes were conditioned at the same time. An active response, in other words, is the essential of conditioning, not merely the association of the stimuli.

11. The certainty of conditioning seems to depend on the number of pairings of substitute stimulus and response.

12. Negative or inhibitory conditioning is possible, achieved by presenting a stimulus and not insuring the response, or by distracting the response, or by inhibiting the response, or by presenting the substitute stimulus after the original stimulus. The signal has a positive inhibiting effect on the response. In many cases such inhibition is clearly accompanied by conflicting responses.

13. Retention of conditioned responses over a period of years has been reported.

14. A conditioned inhibiting stimulus has an after-effect which may last as long as fifteen minutes. Such after-effects are in some cases additive.

15. When a substitute stimulus has been conditioned, other stimuli to the same class of sense organs may be found to elicit the response. This is called *generalization.*

16. If the substitute stimulus is presented some time before the original stimulus, this delay interval finally characterizes the conditioned response.

17. Similar stimuli (as two tones of about the same

frequency) may be discriminated by following one but not the other with the response.

18. A point of resemblance can be reached at which this discrimination will fail and previous conditioning will be lost and a general disturbance of behavior appear.

19. The same disturbances in behavior can be produced by delaying the original stimulus in practice.

20. A stimulus acting separately and the same stimulus acting as an element of a pattern may have radically different effects; the combination may act as a conditioner and the element as an inhibitor; or the element may act as a conditioner and the combination as indifferent. When we refer to a "stimulus" we must ordinarily mean a combination of stimuli in which the pattern is essential. Humphrey (1933, page 156) conditioned a response to a tone and found that when the tone was made part of a melodic phrase the conditioning effect was absent. Josiah Royce illustrated this point many years ago by pointing out that a man's response if we step on his foot and then apologize will be quite different from the response we elicit if we apologize and then step on his foot.

The last item of this list, pointing out that the pattern in which stimuli occur must be considered in speaking of stimuli as a cue or signal for a response, and that the stimuli in this pattern may act without reference to their effects when the elements are separate or arranged in different patterns, is not meant as an endorsement of the Gestalt doctrine that patterns are effective without reference to the particular sense organs they may be acting on. This will be considered later.

Pavlov's experiments required an original stimulus to elicit the response. This was the presentation of food or the introduction of acid into the dog's mouth, and the response was the secretion and discharge of saliva. This stimulus was called by him the unconditioned stimulus. Razran has suggested calling it the conditioning stimulus to indicate its active rôle in bringing about the conditioned response. This name is quite misleading, because the conditioning stimulus is not an essential to the effect. We shall here use the term "original stimulus" to name this already established stimulus when there is occasion to refer to it. The new stimulus which becomes a stimulus for a response through conditioning we shall call the *substitute stimulus*. The essential thing is that the response occur, whether we know just what stimuli are responsible for it or not. If the response occurs, such stimuli as accompany it tend to become substitute stimuli for the response.

In many cases the response is not dependent on any one known original stimulus but on a complex situation in which the most important features are internal to the animal. In a recent monograph, Jensen (1932) reports that moderately full infants that had just stopped suckling resumed in all cases when the infant was dropped four inches, or when the large toe was pinched, or when hair was pulled. Here, nearly any outstanding stimulus produces the reaction, and the specific nature of the stimulus is irrelevant. Anything which will increase general tonus reinforces the suckling movements.

Weiss' students at Ohio State University found (Pratt,

Nelson, and Sun, 1930) that in new-born infants the specific nature of the response could not be predicted from the specific nature of the stimulus. The response which was occasioned by a light, a noise, a touch, was often some response recently practiced, for which the infant was presumably "set." The infant that had been fanning its toes was apt to respond to any of the stimuli offered it by reacting in this manner.

The view which a number of psychologists (including myself) held for a time, that the new-born infant is equipped with very definite reactions which can be set off by a limited group of definite stimuli, must be given up as the result of recent work on the development of behavior in animals and man. We cannot go the whole distance with Holt (1931) and say that all stimulus-response associations are dependent on conditioning. Maturation of the nervous system appears to be the principal determiner of many classes of acts. McGraw (1933) found in giving one of two identical twins elaborate training and comparing results with the untrained twin, that reaching for toys, crawling and standing up alone, sitting erect, grasping and the disappearance of grasping, using a tricycle, all appeared in the two children at the same time without regard to training, whereas climbing, roller skating and other activities could be developed by training. Gesell (1929) has done similar work with twins, reporting a number of activities that seem to depend on maturity rather than on practice.

Even if we grant that much behavior must wait for the normal growth of the nervous system and that some cues

for action are in this sense determined congenitally, conditioning remains the principal way in which the behavior of a person is adjusted to the peculiarities of his environment. If environments were all alike, maturation alone might care for the preservation of life and for propagation. But environments are not alike, and only through developing differential behavior can individuals be adjusted to different, or to changing, environments.

The phenomenon of conditioning is, as Razran has suggested (1930), part of a more inclusive rule. The clasping reflex of a frog is only intensified when stimuli which would cause a resting frog to jump are applied. Fighting dogs fight all the harder for the blows that would ordinarily send them howling away. We are forced to believe that new stimuli, unless they cause responses that break up an activity in progress, contribute to that activity and that this fact is intimately connected with their later tendency to evoke that activity. The conditioning effect, the contribution of new stimuli to an activity, is present on the first occasion, as well as on the second. On the first occasion its presence is demonstrable only through the added energy of the response. On the second occasion the new stimuli may call out the response without those original stimuli which were necessary on the first occasion.

This has a bearing on Razran's principle of dominance. When stimuli are present which would, if there were no competition, call out incompatible movements or movements of opposed muscle groups the results are complex. In some cases, or to some extent, the opposed muscle

systems may be both thrown into contraction, and each group maintain its contraction. We know that a single muscle will respond to stretching or to resistance by heightening its own contraction through circular pathways. The resultant condition is conflict and excitement. But the competition of the opposed movements is not all on this tug-of-war basis. The contraction of one muscle group may result in the relaxation of opposed muscles through central inhibition. Sherrington has demonstrated this phenomenon but there is not much agreement over the nature of the central mechanism by which it is achieved.

In this case, one of the two incompatible movements may inhibit the other and so dominate the other, to use Razran's language. The dominant response will then enlist the stimuli for the other competing response as its own conditioners. The pin prick will become a signal for saliva flow and eating movements; or the food will become a signal for protective withdrawal. Which of these will occur depends on the relative intensity of the competing stimuli, and on facilitation and inhibition from the remainder of the situation. There may result mutual inhibition and blocking without dominance. For this reason I hesitate to burden the theory of conditioning with a principle of dominance, although the principle as stated by Rexroad describes a common sequence of events, and it may be profitable to use the term "dominance" to describe this. The fact underlying dominance is that the response which occurs is the response which will be conditioned, even if this response is (as it always

is to some degree) a compromise movement. The Russian laboratory dog on being pricked and offered food at the same time may eat, but it will eat with a difference. Some movement of avoidance disturbs eating; or it may avoid the pin-prick, but with a backward glance at the food.

The principle of conditioning emerges from all these qualifications in a battered but recognizable form. The behavior we would predict or control, the habit we would modify, the name we wish to remember, the fear we wish to avoid, are not generally clear and definite acts. They are never twice just the same. But they are enough the same to be recognizable, and any light on the conditions under which they occur or fail to occur will be well worth while.

The reader is reminded that the views here presented are to be sharply distinguished from the theory that behavior is a composite of the conditioning of stereotyped reflexes. There are no stereotyped reflexes to begin with. The evidence now available would indicate that the first movements of the young animal are best described as general or massive, and that highly specific responses are, with possible exceptions, developed through conditioning in a manner that will be considered in the chapter on Habits. E. B. Holt's book, *Animal Drive and the Learning Process*, is a clear and thorough exposition of the possibilities of conditioning as the process through which behavior develops. I do not go the whole way with Holt in Holt's assertion that all the behavior currently attributed to maturation is the result of conditioning, nor

do I agree that the physiological basis of conditioning is growth of neurones in the direction of closer contact between active pathways. I am indulging in no physiological theories whatever, and am restricting myself to an effort to describe the phenomena of learning as they can be observed. Holt believes, for instance, that walking is purely the result of conditioning. I would not agree that we can yet give up the distinction between learning and the behavior that depends on growth and metabolism.

Holt's views and my own agree in being radically different from the notion of behavior as composed of unit reflexes. The statement of conditioning here offered, namely, that stimuli acting with a response later tend to elicit that response, says nothing of elementary reflexes. That which is conditioned is whatever complex of movements was in process, not isolated movements or glandular secretion. Pavlov kept his record of the salivary discharge, but recognizes that this is only a small part of the total response which proved conditionable. The term "conditioning" is here preferred to Hollingworth's suggested "redintegration" because the integration of the response, the tendency for behavior to take on stereotyped habit patterns can be understood in terms of conditioning. When a response has once occurred its parts tend to condition each other. Redintegration better describes the total effect, but conditioning is the fundamental event.

Many of the objections to a theory of learning stated in terms of conditioning have been directed at the no-

tion that the conditioned reflex of the Russian experiments is the fundamental type of all learning. The conditioned reflex, as developed in the Russian dogs, is not subject to forgetting to the extent that ordinary learning obviously is. Another objection lies in the susceptibility of the Pavlovian conditioned reflex to temporary extinction; a third objection is that practice seems necessary to establish the conditioned reflex, whereas many associations are established by a single conjunction. These objections are dealt with in succeeding chapters. In my opinion they are valid objections to the notion that the Pavlovian conditioned reflex is the elementary form of all learning; but they are not valid objections to the conception of conditioning here being described. The Pavlovian reflex is not an elementary form of learning, but a complex form. Its peculiarities and its differences from other forms of associative learning derive from the peculiar circumstances of the Russian experiments.

In the laboratory, for instance, the dog is exposed to stimuli which are very different from those he encounters in the kennel. If the kennel were equipped with bells and buzzers, negative adaptation and hence "forgetting" of the conditioned response to these noises would soon take place. As for the need for practice, Pavlov reports (1932) that in the earlier years fifty to one hundred repetitions were necessary to establish the conditioning, whereas improved methods now make only ten to twenty necessary. If it were possible to control the dog's posture and movements or if reactions

using intense stimuli could be used, one pairing would undoubtedly be enough. Concerning temporary extinction it may be here remarked that such extinction is not a fundamental law of the conditioned response, but depends on the circumstances of the experiment, as will be explained in the chapter on Inhibitory Conditioning.

A more serious objection to conditioning as a fundamental principle of learning has come out of some of the experimental work on conditioning. Schlosberg (1932) found that the conditioning of the knee jerk to the sound of a bell failed to occur in some of his subjects in spite of indefinite repetitions, and this is an experience like several reported in our own laboratory. One spoken request to the subject would, of course, have led him to jerk his leg at the signal. This appears to make a sharp distinction between conditioning and voluntary action, and will undoubtedly lead those writers who have a strong prejudice for explaining the simple in terms of the complex to make voluntary action the more fundamental form of behavior and assert that conditioning depends on this.

Further research into the circumstances which favor or discourage conditioning are needed. Failures to elicit the knee jerk to the sound of a bell even after hundreds of pairings may lead to the discovery that the substitute stimulus must be responded to on its own account in order to establish conditioning. Reasons will be given in the following chapter for believing that it is not the bell but the movement-produced stimuli of the response

(such as listening) to the bell that are the effective conditioners of the conditioned response.

The outstanding result of conditioning is the anticipation of natural events which have been experienced before. The sight of the bitter caterpillar first moved the chick to peck it. The bitter taste (for which we have the word of the entomologists) causes a vigorous rejection. On a second encounter the rejection takes place before the peck; the chick has what Lloyd Morgan has called a foretaste of the caterpillar and now does its rejecting in time to avoid embarrassment.

What does the principle of conditioning mean in the form of practical advice? Largely this, that if we wish to have any act of our own or of another under our control so that we can elicit it on occasion, we must go through the following procedure: First the act must be somehow or other elicited or simply awaited. If we know an effective stimulus for this, well and good. We must at least know that the act is within the animal's or the person's repertoire. We can not teach cows to retrieve a stick because this is one of the things that cows do not do. It is because dogs' behavior includes chasing sticks, taking objects into the mouth, walking and running, that we can build these into a conditioned response to a signal. At the beginning of the act we may speak a word or make a movement which will in time become the cue for the act.

If we wish to teach a dog to come when he is called, our method will be to get him to come to us by hook or crook. There are no rules for this except what we know

of dogs in general. We may hold up a bone, start running away from the dog, or pull him toward us with a check line, or use any device which experience has suggested. While he is coming we speak the dog's name. If we take care not to speak the name on any occasion when we foresee that he will not come, when he is, for instance, chasing a cat or gnawing a bone (when we believe an unwanted response is dominant), we can readily establish a stable conditioned response. We say the dog "knows" his name. If we are so misguided as to try to call him back from the pursuit of a passing car before we have insured the effectiveness of calling, we have reconditioned the dog and made his name a signal for chasing cars, not for coming to us.

The skilled trainer uses his dog's name only when the prompt response is highly certain. If the response fails, he does not repeat the name, but uses his practical knowledge to remove the cause of failure or waits until the cause is removed. The dog may have been occupied in looking at another dog or watching a passerby. The trainer waits until he has the dog's attention before he repeats the name. Otherwise the name tends to become a cue for looking at the passerby or noticing other dogs.

It is on exactly the same ground that the student officer is cautioned never to give a command that he is not confident will be obeyed. If the command is followed by acts other than those commanded the command becomes merely a cue for disobedience and the officer loses his authority.

If we are ourselves concerned to learn to address a

new acquaintance by name we can achieve that result by following a simple rule: Speak his name while looking at him. Social convention will prevent our using a method which would insure remembering. This method would consist in shouting his name at the top of our voice while looking at him.

A memory for names consists in very simple habits of using the rule of conditioning. The person who excels at it has a settled habit of using names in conversation while looking at his victims, or of rehearsing them subvocally under the same circumstances. The reason most of us fail is that we do not look at the man while naming him. The person with the memory for names takes occasion to do this as often as he can manage: "Yes, Mr. Walker. . . . No, Mr. Walker. . . . Don't you think so, Mr. Walker?"

An examination of the methods of the successful animal trainer or of the well-written practical book on training will find these can be translated into terms of conditioning with astonishing ease.

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Chapter IV

TIME FACTORS IN CONDITIONING

Two small country boys who lived before the day of the rural use of motor cars had their Friday afternoons made dreary by the regular visit of their pastor, whose horse they were supposed to unharness, groom, feed and water and then harness again on the departure. Their gloom was lightened finally by a course of action which one of them conceived. They took to spending the afternoon of the visit re-training the horse. One of them stood behind the horse with a hay-fork and periodically shouted "Whoa" and followed this with a sharp jab with the fork. Unfortunately no exact records of this experiment were preserved save that the boys were quite satisfied with the results. The experiment, if it could be repeated on a large scale with many horses, could have been so managed as to throw a considerable amount of light on a very important problem in conditioning. What interval between the shout and the jab would result in the most efficient learning of the association? How great an interval could there be and association still occur? Could the shout have been given after the prodding and have the sound become a conditioner of the vigorous lunge that the boys actually succeeded in attaching to the word?

Will we learn the French equivalents of English words more rapidly if we first read the French and then the English, or the other way about? How long can a punishment be delayed after the crime and still have a deterrent effect? Will we have to induce the burglar to re-enact the crime and punish him just as he performs, or even a bit before he begins his burglary?

These questions all concern the time relations between cue and act. The answers to these questions have been made very difficult by an accidental bad start in the method of studying the conditioned response in the laboratory. Most of the text-books of general psychology give the answer thus: Conditioning will be most certain if the cue is given along with the original stimulus; the cue can be given somewhat before the original stimulus and still have some effect; but the cue cannot be given after the original stimulus and have any conditioning effect.

The essential mistake of this description and of the experimental work based upon it lies in viewing the two stimuli, the original stimulus and the substitute stimulus, as the associated items. It is the time relations between these stimuli that are observed and recorded. But the time relations between original stimulus and substitute stimulus are of no particular importance except that they are fairly easy to record. *It is the time relations between the substitute stimulus and the response that count.* It is not the two stimuli that are associated, but the substitute cue and the resultant act. The experiment by Harlow and Stagner already mentioned has some bearing on this

statement. They used an electric shock to cause their laboratory animals to jump vigorously. Associated with the shock a buzzer or a sudden light readily became substitute stimuli for the response. But in animals which had been given curare, which interferes not with nerve conduction nor with the contractility of muscle but which blocks the connection between motor nerve and muscle and hence paralyzes the animal, no conditioned jump could be established.

These curarized animals, which had received the shock without making any jump (because their skeletal muscle was paralyzed) were given the cues, the buzzer or the light, when they recovered from the effects of the curare. They showed a conditioned pupillary dilatation (smooth muscle like the iris of the eye is not affected by curare) but no sign of a conditioned response in striped muscle. The original stimulus, the shock, and the possible substitute stimulus, the light or the buzzer, and probably the sensory nerve impulses from both original and substitute stimuli had occurred together, but no association had resulted.

Not only curarization and sleep, but also fatigue has the effect of diminishing or eliminating the conditioned response. (Cf. Bykow, 1927.) Not the original stimulus, but the response, is the essential of conditioning. If the response fails, conditioning does not occur.

Pavlov used for the original or unconditioned stimulus food or acid introduced into the mouth. There is some evidence that in human beings the movements of chewing cause salivary secretion. In case this is also true of

dogs, the original stimulus of food becomes somewhat questionable as the true occasion for salivary flow. Food might start chewing, and chewing start secretion. If we have measured or recorded the instant at which food is offered, this may have been separated from the actual stimuli for saliva by a series of events and one of the later parts of this series is the real occasion for secretion.

Hollingworth has pointed out that for most responses there is no one original stimulus. The response is the outcome of a large complex of stimuli acting on the animal. He discarded the term "conditioning" as too much committed to definition in terms of an original stimulus. His principle, which he calls the principle of redintegration, is to the effect that after a response has occurred, a part of the original situation present may re-establish the total response. The writer's formula is very similar to this: Stimuli acting at the time of a response tend on their recurrence to evoke that response. But this formula implies definite simultaneity, whereas Hollingworth is noncommittal on that point. There is good reason to believe that in the time relations of cue and act, definite simultaneity is essential for conditioning. The cue must accompany the act if the association is to be formed.

If we follow the method of most of the experimenters and observe the time relations of original and substitute stimuli, there is clear evidence that the two can be separated in time by a considerable interval and association result. Pavlov has reported such separation effective for intervals as long as thirty minutes.

If a bell is rung and then, after an interval of, say,

two minutes, food is presented with resultant secretion of saliva, and if this sequence is practiced over and over, the result will be that the cue can be sounded and, after a two-minute interval, saliva will flow whether food is presented or not. Pavlov calls this a trace reflex. If the signal is continued throughout the interval he calls it a delayed reflex, though the difference between the two is not fundamental.

Even backward conditioning can occur. The signal can be given after the original stimulus and still become an effective conditioner. Whether the boys with the preacher's horse could have achieved this with the sequence, first prod, and then "Whoa," we unfortunately do not know. But Wolfle (1932) found something very like this to work. She had an apparatus which gave the fingers a shock; on receiving this the hand would be jerked away from the contact. Sounding a buzzer before the shock or after the shock (backward conditioning) both gave some conditioning. The most effective method was to sound the buzzer three-tenths of a second before the shock, which resulted in conditioning in fifty-eight per cent of the cases. Giving the sound just with the shock at short intervals through a forty-minute period resulted in ten per cent of conditioned responses when the sound was given alone. Giving the sound one second after the shock (backward conditioning) resulted in an occasional conditioned response to the noise.

Experiments with association also show that the cue and the original stimulus can be separated by long intervals and the association still be effective. In one ex-

periment in the association of a nonsense syllable and a nonsense figure (Guthrie, 1933) the syllable is thrown on a screen by an automatic projector; there is then an interval of darkened screen and this is followed by the projection of the shape. The intervals between stimuli could be varied. The object is to make the odd figure the cue for pronouncing the syllable with which it was associated. This is backward association, since the original stimulus is shown first and the stimulus that is to become the cue is shown later. The associative strength as shown by the per cent of the list of syllables correctly named when the figures alone were displayed turned out to be greater at an interval of 4.9 seconds than for 2.5 seconds.

Forward association, curiously enough, was of almost the same effectiveness as backward at corresponding intervals; and had no advantage. In Pavlov's experiments where maximum conditioning effect is wanted, an interval of about two seconds is used and the substitute cue given first.

How can it be maintained, in the light of such results, that conditioning and association are actually simultaneous? My own answer to this would be that the substitute stimulus which is made a matter of record is very seldom the actual immediate stimulus for the conditioning. If it were, Cason (1924) would probably be right in claiming backward association to be an impossible sort of event. How could a stimulus come to be attached to a response that is over and gone? Or how could a stimulus become attached to a response that is not to take place for five

minutes? But if the bell, or the light, or whatever is used as the substitute cue is not the true conditioning stimulus, what is?

The answer is that there are other stimuli in plenty to take that rôle. When a bell is rung or a light is flashed or any new and emphatic stimulus applied to an animal, the animal will respond to that stimulus. It will listen to the bell; in response to the light it will move its head and eyes; at a pin prick it will move away its body or its limb; at a smell it will sniff and turn its head. Every such motion is a stimulus to many sense organs in muscles, tendons, and joints, as well as the occasion for changing stimuli to eyes, ears, etc. We may call these *movement-produced* stimuli, for the reason that they are produced by our own movements.

Such a movement as listening or looking is not over like a flash or an explosion. It takes time. The movement, once started, maintains itself by the stimuli it furnishes. When the telephone bell rings we rise and make our way to the instrument. Long before we have reached the telephone the sound has ceased to act as a stimulus. We are kept in action by stimuli from our own movements toward the telephone. One movement starts another, that a third, the third a fourth, and so on. Our movements form series, very often stereotyped in the form of habit. *These movements and their movement-produced stimuli make possible a far-reaching extension of association or conditioning.* They make possible remote association in which the remoteness is only limited by the length of such a regular series of movements. The

nervous impulses directly resulting from the sound of a bell are over in a fraction of a second. They travel in nerve trunks as fast as 400 feet in one second, and in a very tall man they have a comparatively short distance to go to any muscle. Strictly speaking, we are answering the bell only for the first half second or so; after that we are answering our own actions. In this case, of course, the series of movements set up is not perfectly stereotyped. We avoid the edge of the table, reach for a pencil, pick up the telephone in response to new "outside" stimuli, but not only in response to these outer stimuli. Our own movements are our chief guides. The effects of the bell may reverberate through our whole future; but the direct impulses from the sound are soon over.

To return to the delayed or trace reflex—here the real direct conditioner of the salivary flow was not the signal given by the experimenter, but the movement pattern of the resulting series of listening movements which happened to be simultaneous with the discharge of the salivary gland.

Pavlov reports that during the waiting period of a delayed reflex new stimuli may either prevent the expected response or lead to its premature release. How are we to account for this unless we assume that the new stimulus has broken in on the serial response initiated by the cue, and may either prevent the actual movement-produced conditioner from appearing, or, in rare instances, hasten it?

It is only fair to the reader to warn him that Pavlov, the pioneer experimenter in this field and the scientist

who has undoubtedly the most extended acquaintance with the phenomena of conditioning has sharply disagreed with me concerning the rôle of these movement-produced stimuli in producing delayed and backward conditioning. One of the experiments which Pavlov cites (1932) in his argument furnishes, to my notion, a beautiful illustration of the possible rôle of movement-produced stimuli in timing the response.

The experiment is this: A conditioned reflex is formed by giving a signal, such as a buzzer. There is then a short interval of waiting, followed by administering food. After this sequence has been practiced a number of times there appears the ordinary conditioned reflex; the dog, on hearing the buzzer secretes saliva.

Then the program is changed. The interval is lengthened to a few minutes instead of a few seconds. The conditioned reflex quickly disappears when the food no longer follows at the practiced short interval. As the changed program is repeated there is gradually formed a new conditioned reflex with a latent period of minutes instead of seconds. Pavlov does not say that a new reflex is formed, but that the reflex "reappears." It is my belief that the two-second reflex was conditioned to some movement pattern that followed the buzzer, not directly to the buzzer. When the method is changed this conditioned reflex disappears, as such reflexes will if not reinforced with food, and another reflex conditioned on a later movement pattern, the one coinciding with the belated food, is established.

These delayed and trace conditionings are familiar in

everyday experience. A good drill-master always gives his commands in the same tempo. After his preparatory command the command of execution must follow at just the practiced instant or there will be conditioned and premature movements of execution. Where traffic is changed by a double bell signal, a delay of the second signal will find many drivers starting without it. Keeping time with the world is so familiar that our illustrations are striking only when something has gone wrong. The sound of a footstep leads us to expect a visitor after an interval; the musician resumes playing after a musical rest without disturbing the rhythm; Mark Twain describes the tense anxiety produced by the failure of the retiring hotel guest in the room above to throw on the floor his second shoe; we have all waited breathlessly for the inexpert pianist next door to play the delayed chord.

The timing of a delayed response to a stimulus may be astonishingly exact. The clock mechanism by which this timing is done is not to be looked for where Pavlov seeks it, in obscure delays in the cerebral impulses. It is not necessary to suppose that brain nerve-cells can hold up impulses and release them when ready, possibly two minutes later. There is a much more obvious method by which this timing can be accomplished. It can depend on peripheral movements and the sensory impulses caused by these movements.

There may exist some cerebral rhythms. Sherrington believes that the rhythm of a dog's scratch reflex is centrally determined and is independent of the return impulses from the muscles. I am not at all sure that the

same results would be found for such a movement as tail-wagging. Do those dogs that have been relieved of the mass and length of a long tail by early operation wag faster than dogs that carry this burden? If they do it is probably because movement to the right has to wait until movement to the left has given the cue.

A short generation ago there were many experiments comparing the relative effectiveness of simultaneous and successive association. The experiments were a no-decision affair as they should have been, because the results depended on the opportunity for simultaneous conditioning offered by the particular activity studied. Pavlov and many others (Borovski, 1927) have reported that maximum conditioning occurs when the cue is given from .2 to 2 seconds before the original stimulus. In less than two-tenths of a second the sensory nerve impulses have probably had all the direct effect that they ever will have, and it is not their direct effect on the brain, but the effects of their effects, the movement-produced stimuli from the movements they start, that serve as the release of the conditioned response when it comes.

Backward conditioning is probably like forward conditioning in depending on the actual coincidence of the real cue and the response. It is ordinarily harder to establish in simple reflexes than is forward conditioning. That it can be established at all probably depends on the fact that no normal contraction of muscle in behavior is instantaneous, and the substitute stimulus for contraction can be established as long as the contraction is taking

place. This often lasts for some time after the original stimulus has stopped.

When the contraction is over, the chance for conditioning is lost. Cason (1922) found it impossible to condition a wink to a cue given after the wink was over. Krasnogorski is quoted by Pavlov to the effect that giving the cue two to ten seconds after administering food brought no conditioning even when repeated a thousand times. There was in Krasnogorski's experiment no record of the time relation of the cue to the actual response, however.

Most movements are not so episodic as the wink, and hence are more susceptible to backward conditioning. And some behavior involves the repetition of movements, so that conditioning can take place long after the original stimulus. This is true in experiments on association which use speech. In our own laboratory (Guthrie, 1933) we found that a nonsense figure exposed on a screen 4.9 seconds after a name had been exposed became, after some practice, a cue for the name. It became a more effective cue than when the interval was only 2.5 seconds. None of the experiments on backward conditioning of simple movements found conditioning at such long intervals. I am convinced that this remote association was possible because the name was still being responded to when the figure was shown. The syllable was being "held" or in some cases being repeated as the figure appeared on the screen. Most of the subjects reported that they could thus "hold in mind" the figure or the syllable for some seconds. Of course they were hold-

ing it in their muscles as well as in their quite problematic minds, but their report was substantially correct.

In associative learning involving speech or thought, conditioning loses its clear time relationships. It is so difficult to observe the movements that accompany thinking that we cannot know just when the response is present and when not. Original stimulus and substitute stimulus may be separated for days, and their association depend on mediating associations. This is true in cases where delayed punishment is effective. When a parent spanks a child for writing on the living-room walls an hour after the offense and in a different room, the only possibility that the punishment will establish a new response to walls and pencil in hand is that speech is so well established in the child that it can conjure up by its associations the tempting situation. Through language the child is stood again before the wall with a pencil, and by the spanking the original tendency to write is displaced by other tendencies.

We may be told that the name of the man we met yesterday is Wilberforce, and remember this when we next see him. The phrase, "the man you met yesterday," serves to reinstate our perceptual response to the man, and with this is associated the name. When we come across him the perceptual response occurs and now serves as a cue for the name.

Robinson has suggested (1931) that the law of association by contiguity should state associative strength as a function of the interval between the cue stimulus and the original stimulus. This function would be graphically

represented by a curve rising from the very low values for backward association to a maximum for simultaneous association. It would fall off more gradually as the interval increases in forward association.

This curve of Robinson's fits very well the experimental results of conditioning simple movements, except that maximum associative strength is found when the substitute cue is given a fraction of a second before the original stimulus, and not when the two stimuli are given together. But Robinson's curve does not at all fit the results of experiments in word-association like the experiment of my own described above. The type of curve depends on the nature of the material used in the experiment, and ultimately on the chances for overlapping of cue and response.

The reason why forward association is so much more effective in experiments with animals and with simple movement in human beings is that the cue or signal used in the experiment may start its own movement series of indefinite length, and any phase of this movement series may act as a simultaneous conditioner when the response takes place. We get better results with short intervals because we can depend on the movement series to be more stereotyped for a short interval.

Backward association, on the other hand, usually depends on the prolongation of the movement conditioned, and so the cue must often occur within the space of a second. If we wait longer, the response is no longer there to be conditioned. In experiments with word association we have to do with very complicated learning. Attention

is more precarious. We cannot be as sure that a subject really "looks at" a word or a figure on a projection screen as we are that he jerks his hand away from a shock, or that he hears the sudden noise of a buzzer. And in word material the subject may continue to form the word soundlessly, or continue to perceive the figure after the word and figure are gone. The subject may be rehearsing inner speech without our knowledge. We find the curve of relationship between associative strength and associative interval in this case quite different. There is no general curve for the relationship between associative strength and associative interval because different activities offer differing opportunities for simultaneous conditioning.

We may summarize our findings on the question of the time relation between a conditioned signal and its response: The actual associative process is probably always dependent on a precise coincidence of the cue and the response for which it becomes a cue. This requires, however, the assumption that the actual cue is seldom the observed and recorded one. The recorded cue has started a series of events, and one of these unrecorded and usually unnoticed events is the signal directly responsible for the nerve impulses that begin the movement.

Our theory then cannot give clear advice about such questions as were asked at the beginning of the chapter. It becomes necessary to know something about the nature of the activity before we can say whether a long or a short interval between the signal and the desired re-

sponse will be more effective; or before we can say whether the signal should be practiced before or after the original stimulus. It is possible to say that in a simple movement like the flexing of an arm, signals have a maximum effect when given a fraction of a second before the response. The length of this time interval would indicate that the effective conditioner is not the signal given, but the response to that signal. In our illustration at the beginning of the chapter the farm boys in their sinister training of the pastor's horse undoubtedly followed this procedure and shouted "Whoa" an appreciable time before applying the fork. It is to the horse's perceptual response to "Whoa" and not directly to the word that the response is attached. In human behavior, many of our acts are conditioned, not directly on the external cues, but on the perceptual response to these cues. In verbal association, these perceptual responses can be so maintained and repeated that the underlying simultaneity of the actual conditioning is effectively hidden from observation.

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Chapter V

INHIBITORY CONDITIONING

We break habits as well as form habits. Under what circumstances does a conditioned stimulus cease to call out its response? Learning any skill depends as much on getting rid of awkward movements and useless movements as it does on acquiring the proper movements to the proper cues. How do we lose hates and loves, fears, and indignations, our memory of friends' names, our typing errors, our enthusiasms? What rules can we follow to rid ourselves of annoying habits? Can such rules as are offered us fit into our general theory of learning?

The facts are that a stimulus which has, to use Pavlov's language, been conditioned to a response may become not only "unconditioned" or "de-conditioned" but an actual discourager or inhibitor of a response. We shall use the term *distraction* to describe the effect of diminishing or partially impeding a response; for the prevention of a response, or of its overt expression, the term *inhibition* is to be used. Inhibition is almost never complete. Even when overt expression is prevented there is usually discernible some minimal expression of any response for which cues have been received.

How do stimuli become distractors or inhibitors of an

action? We may recall that in the case of the pastor's horse the sound of the word "Whoa" had previously been a signal for stopping. This had been in its turn the effect of training in which the horse had been checked by the rein and the sound uttered a second or so before. The boys' efforts had substituted another reaction for the conventional one and in effecting this substitution the word has become an inhibitor of the first response.

A stimulus which has become a conditioner of such an act as turning the head to the right, or dilating the pupil of the eye, or of any movement whatever, will necessarily inhibit turning the head to the left, or contracting the pupil, or performing movements incompatible with its conditioned response. Turning the head to the right physically prevents turning it to the left, but that is not all. Physiologists, notably Sherrington, have demonstrated in many instances the existence of a reciprocal innervation which brings it about that the contraction of one set of allied muscles tends to relax the muscles opposed. This interference is accomplished through central connections which allow flexor muscles, through their muscle sense organs and central nervous system connections to deprive the extensor muscles of impulses or allow the extensors to have the corresponding effect on the flexors.

When rival movements are both in process, the outcome of the rivalry is in some cases undoubtedly a matter of the relative strength of the opposed movements, but in most cases it is also a question of priority, much as at a grade crossing the train which reaches the

crossing first sets a derailing switch against any competing train.

A stimulus may thus be unconditioned by the very simple means of becoming a conditioner for an incompatible movement. Unlearning becomes merely a case of learning something else. And the rule which states whether conditioning or unconditioning will occur becomes simply the familiar principle of conditioning: Stimuli which are acting at the time of a response become conditioners of that response. In this case, *the response referred to in the rule is a response incompatible with the former response.* The horse cannot lunge forward and stop at the same time. This is physically and neurologically impossible. The signal inhibits stopping because it has become alienated from that response by a later association with the incompatible response.

In his typical experiment Pavlov has accustomed the dog to the experimental room and apparatus. When the experiment begins after several days of this introductory training the dog is stimulated by the ringing of a bell, and this is followed in perhaps two seconds with the presentation of food. When the bell rings the dog pricks up its ears and turns its head or makes other movements which we may describe as listening. The presentation of food interrupts these movements and causes the dog to turn to the food and eat. We have been in the habit of describing these responses as (1) listening and (2) eating, and of speaking as though the responses both maintained their original character after they had occurred together. This is a misleading description, because it at-

tributes a false simplicity to the event. In reality, after their conjunction, neither response is the same. The two responses cannot take place in the same dog and fail to affect each other. Eating may supplant listening when the food is administered, but some traces of listening behavior persist. The resultant response is not an exact re-instatement of either listening or eating. And it is the resultant that is conditioned.

Pavlov states (1932) that the listening movements gradually disappear, so that no trace of them finally remains after a number of practice periods, and the bell causes the dog to lick his chops and stand ready to eat. This statement calls for a little skepticism. The gradual disappearance of listening may leave many traces in the final behavior of eating which are not conspicuous but still are present.

Why should listening tend gradually to disappear? What parts of listening disappear first? Razran and Rexroad would answer this in terms of a principle of dominance. In Rexroad's words, "A stimulus gains effectiveness for a given response when the stimulus is followed by that response as a dominant response." I believe the words, "as a dominant response," are here unnecessary. The stimulus gains effectiveness for whatever response or mixture of response it is that follows.

The answer to the question why listening tends gradually to disappear is that some of the movements of listening and of eating are incompatible. Food and the dog's hunger in causing the dog to eat cause him to stop listening, at least to end such motions of listening as are

incompatible with eating. Listening becomes a cue for eating plus such traces of listening as have not been inhibited by eating. In so far as the two acts are incompatible, eating has won. The outcome might have been very different. If the noise of the signal had been so loud as to inhibit the movements of eating and cause the animal to struggle in the apparatus, the administering of food would have become a cue for fright and struggle.

The response that prevails has the advantage. Tensions in the opposing muscles of the submerged response were overcome, some of them physically, by superior force, and some of them through neural inhibition. On the next occasion such inhibition tends to occur earlier, to be anticipated, for this is a normal result of conditioning. The result is that one response or the other, in so far as they are incompatible, finally prevails more and more thoroughly and enlists the stimuli for the submerged response as cues for the prevailing response. But the final conditioned act is not a replica of either original response.

It will be seen from this account that the Pavlovian conditioned reflex is by no means an instance of simple conditioning in the sense in which that word is used in this book. Pavlov's conditioned salivary reflexes are to be explained in terms of conditioning, but they are not the elementary forms of conditioning on which to base a theory. There are no such elementary cases of conditioning in an animal of any complexity because its behavior is not to be described in terms of a limited number of elementary responses or reflexes which can be named

and numbered and easily identified. The very process of conditioning confuses and mingles responses into new combinations. The behavior of any animal is a total integration in which we recognize and name occasional familiar details. Our only hope of predicting behavior, and hence our only hope of a theory of learning lies in describing the approximate circumstances in which such recognizable details will occur. It is our present contention that the circumstances under which responses recur include stimuli present when the response last occurred.

It is quite consistent with this conception of conditioning to describe *inhibitory conditioning* as the conditioning of some inhibitory response. A stimulus which has in the past contributed to one set of movements may cease to do this and may act as an inhibitor of such movements when it has been associated with another response. The loss of an associative connection with a response was called *negative adaptation* by Stevenson Smith and me in our *General Psychology*. At that time we suggested three ways in which inhibitory conditioning could be brought about. *The three ways all involved the presence of the cue and the prevention of the response.*

A conditioned stimulus may be acting and the response fail simply because the stimulus is below the threshold; or the response may be eliminated through exhaustion or fatigue; or the response may be inhibited by the action of incompatible responses. In any one of these three cases a stimulus is present and a certain response fails to occur. Other responses do always occur.

The result is that the stimulus conditions the other responses and is thus an inhibitor of the response in which we were interested.

To these three methods of establishing inhibitory conditioning by insuring the failure of the response there may be added a fourth, to which Dodge has called attention: the substitute stimulus may be applied again so soon after its response that the response is still in what is called its refractory period. The evidence is that most responses have such a refractory period. Immediately after the movement has taken place there is an interval during which the movement cannot be elicited; following this there is a period during which the stimulus must be of greater than normal intensity in order to elicit the response. This is called the relative refractory period and may also show itself by a diminished response to a normal stimulus. This method of establishing inhibitory conditioning is, like the others, the result of applying the stimulus under circumstances which prevent the response. The conditioned stimulus loses its connection with the former response because it has become attached to other behavior.

A bitter taste may be so slight that it fails to cause ejection of food. Such a taste may be introduced into food very gradually and the result be a toleration of a degree of bitterness which would have been at first out of the question. This is an instance of the first method of negative adaptation. The bitter taste has been present in increasing degree. It becomes a conditioner of appetite and eating, and may eventually be necessary to the en-

joyment of some foods. In the same manner we may grow adapted to the pain of fatigue or late hours, or to cold temperatures, new diets, changing conditions of life, without once having been disturbed if the changes are gradual enough.

Whitford's *Training the Bird Dog* suggests how the trainer may bring about negative adaptation to the noise of the gun. Training should commence with the discharge of a cap pistol at a great distance from the dog, and without any unusual movements on the trainer's part. Later a louder pistol may be used and a shorter distance. This may be followed by light loads discharged from a gun (pp. 76-79). If a dog has by bad training or unfortunate combination of circumstances been made gun-shy, re-training is a long and tedious affair, and not ordinarily worth the trouble.

I once asked an old cavalry sergeant who it was that "broke" the new horses furnished the regiment. His language cannot be repeated here, but it was to the effect that no horse-breakers were used in the army. The method used is the first method of negative adaptation described. The trainer keeps always within the threshold of tolerance of the animal. First a light blanket, and then possibly a sack with a little grain is put on the horse's back. At no time is the horse so startled that it plunges or struggles. In time the new burdens and harness are added until the horse is accustomed to the weight of a rider and to the bridle without ever having been stirred to excited resistance. The method of the western ranches was often very different. The wild horse was forcibly

saddled, bridled, and mounted. The rider "stuck out" the resultant struggles until the horse was too exhausted to struggle more. At this stage the disturbing stimuli are present but are not reacted to, and negative adaptation is taking place. The disadvantage of this method is that traces of the first learning in which the rider and equipment are responded to with struggle are apt to survive the breaking. Reconditioning is not complete.

Thus inhibitory conditioning may be brought about by furnishing the stimulus situation when fatigue or exhaustion has stopped the response as well as by increasing the stimulus and keeping it always under the threshold or within the tolerance of the individual. In both cases the stimulus becomes the conditioner of other responses and so an inhibitor of the undesired response. Many a speaker has never suffered from stage fright because his introduction to public appearance has been so gradual that excitement has never appeared. Others, perhaps the majority, recover from stage fright only through being forced to go through with their performance and to continue after the excitement has passed. If they give way during the excitement, positive adaptation occurs and they become incapable of composure before an audience. If they continue on many occasions through the excitement and after it has ceased, the sight of the audience no longer disorganizes their behavior. It is during this period that negative adaptation is taking place. The stimulus situation which had caused excitement is present but the excitement has been replaced by other behavior. The stimulus situation now conditions the new behavior.

The third way in which inhibitory conditioning or negative adaptation may be brought about is through the presentation of a stimulus at a time when its response is inhibited by other elements in the situation. Here also the stimulus is present, but other responses are present shutting out the former response and the stimulus becomes a conditioner of these and an inhibitor of its former response. Whether or not the child comes at his mother's call depends on what he is doing as well as on the loudness of her call. By calling him only when he is occupied any child can be promptly negatively adapted to his mother's voice.

Whether a student will be able to read in the bustle of a library room depends on his early experiences there. If he begins with an engrossing book he will reach a quick adaptation to the noise about him. If he begins by noticing what is going on in the room, the open book will eventually be a mere cue for looking about. The city dweller when he visits the country finds that he was dependent on the city traffic noises for his sleep and lies awake through a quiet night. One wakeful night or two, however, and his trouble is over. Fatigue makes sleep more imperative and while sleeping in country surroundings, these surroundings become the new conditioners of a good night's rest. Bachelors often enjoy negative adaptation to rattling windows and dripping faucets and sleep peacefully through such disturbances; after marriage and enforced nocturnal investigations of house noises they find that sleep is impossible while the noise continues.

Dodge is responsible for pointing out (1927) that in-

hibitory conditioning may be achieved by presenting the cue during that brief refractory period after a response when the response cannot be elicited. This also falls under the rule that when the cue occurs but the response *is by any means prevented*, negative adaptation of the response to that cue takes place. After the knee-jerk, Dodge reports (1931) a second stimulus of the same intensity elicits a second reaction averaging two-thirds the amplitude of the first. The refractoriness of the lid-reflex lasts two or more seconds. "Direct experimental evidence," he says (1931, page 18), "for refractory phase in neural systematizations higher than the reflexes has hitherto been conspicuously inconclusive. This is especially true of cerebral systems. There is, however, a strong theoretical presumption that some barrier to repetition analogous to refractory phase is a post-stimulation phenomenon of all neural tissue. Such a presumption is congruent with many observed facts in behavior and consciousness, but there are few experimental techniques for demonstrating it." "It is," he suggests, "apparently combined in some unknown way with a longer process of negative adaptation." Whether this refractory phase, demonstrable in nerve and in simple reflexes, has any bearing on our disinclination to repeat more complicated behavior immediately is not, of course, at all clear. It would be somewhat absurd to account for a small boy's unwillingness to respond to an immediate second request for his name by calling this a refractory period. This may be a very different brand of refractoriness.

Many psychoanalytic cures are undoubtedly instances

of the third form of inhibitory conditioning. Pathological fear or disgust is felt on speaking or thinking of some situation. The psychoanalyst leads the patient to speak of the critical event under carefully controlled conditions which, through the prepared response of the patient to the analyst, tend to inhibit the obnoxious emotional response and to condition the situation to such substitute responses as prevail in frank conversation. The prominence of the erotic in psychoanalytic method does not indicate that the causes of the neurosis are uniformly erotic in nature, but that sex is a topic of rather universal interest and hence an easy and practical general approach to the establishment of the responses and attitudes which are to be substituted for the neurosis. The occasions for the neurosis may be rather remote from sex and peculiar to the patient. In a country where food is scarce it is probable that the approach to the cure could be as readily established through conversation about food as through conversation about sex.

In other words, the analyst uses erotic conversation for its distracting and finally inhibiting effect on the neurotic behavior, and achieves, in many cases, the reconditioning of the disturbing topics and incidents to harmless response. The same process is involved when we recount an embarrassing incident to our friends and their laughter at our discomfiture causes us to laugh and the memory of the experience loses its power to shame us. A case still more elementary is the effect of a lollypop on a crying child. In psychoanalytic practice, erotic conversation serves in place of the lollypop for the distraction

and inhibition of the undesired response and furnishes a chance for re-training.

We may choose our final illustration from the laboratory. Razran quotes (1933, Archives article, page 86) an experiment by Slutskaya in which children were pricked with a needle and formed a voci-motor avoiding conditioned response to the sight of the needle. This was followed immediately by feeding. In three out of five cases in normal children the conditioned stimulus for avoidance became a conditioned stimulus for swallowing or eating movements. The needle had become an appetizer.

Unmaking a bad habit is thus essentially the same process as establishing a good habit. Bad habits are broken by substituting for them good habits or innocuous habits. The rule for breaking an undesired conditioned response becomes this: So control the situation that the undesired response is absent and the cue which has been responsible for it is present. This can be accomplished by fatiguing the response, or by keeping the intensity of the cue below the threshold or by stimulating behavior that inhibits the undesired response. If the cue or signal is present, and other behavior prevails the cue loses its attachment to the obnoxious response and becomes an actual conditioner of the inhibiting action.

Many an adult who suffers from "cat fear" has been thus re-trained by tolerating in the house a kitten so small and helpless that the fear is not called out. The kitten's growth is so gradual that habits of caring for it and tolerating it, petting it, persist even when it has reached maturity, and the patient finds that cats no

longer call out panic. A psychoanalytic treatment is essentially the same process. An effort is made to recall the circumstances under which the original conditioning of panic occurred. The circumstances recalled, or re-enacted, which is much the same thing, include many of the conditioners for the panic which are also called up by the actual presence of cats, and which are a necessary condition for the production of the fear. To these, under the careful management of the analyst, the patient responds without fear. As a result, the sight of a cat or mention of a cat may become merely cues for talking about cats or talking about the original incident.

Our embarrassing moments continue to embarrass us in retrospect until we have recounted them to ribald or unsympathetic friends, whose laughter has helped to make us laugh. From that time on, they become not occasions for shame and humiliation, but conversational anecdotes. Our dining mishaps, our unfortunate remarks to the hostess, our encounters with the highway patrol in which we played so meek a part, are turned to assets instead of liabilities by being "made light of." Emergencies that require action drown our griefs, soften our humiliations, wipe out depressing memories. Laughter has an important part in social adjustment, in substituting for tension and anger, irritation or embarrassment. And laughter plays this part because it alienates from stimuli their unhappy responses and enlists them as conditioners for laughter. Unfortunately, our ambitions as well as our troubles can be "laughed off."

This ends, for the time being, our account of condi-

tioned inhibition and negative adaptation, which are two names for the same phenomenon. There is another characteristic of the conditioned response which resembles conditioned inhibition but with a difference. For the description of it we are indebted to Pavlov. A substitute stimulus may be detached from its response *temporarily*. If the salivary secretion of a dog has been conditioned on the sound of a bell, the repetition of the ringing at short intervals causes less and less reaction until secretion no longer takes place. But the extinction of the response, unlike the conditioned inhibition described above, is not a lasting extinction. After an interval, the conditioned secretion again appears. If the process of extinction is repeated, the recovery is less and less complete until finally complete negative adaptation is present. Moreover, the extinction may be carried beyond the zero point. If the cue is sounded after secretion has ceased to occur, the cue becomes an active inhibitor of secretion and will prevent or retard secretion when another stimulus for secretion is applied.

Pavlov and his followers established that this temporary extinction is not a result of fatigue of the gland because the gland will still respond to other conditioners as before. Pavlov and several Russian writers look upon temporary extinction as quite distinct from inhibitory conditioning. By several American writers it has been regarded as evidence that the conditioned reflex is a very special and peculiar form of learning and not, therefore, to be accepted as the basic form of all learning. Other

associative learning, they argue, cannot thus be temporarily extinguished.

I do not agree with either of these opinions. I believe that temporary extinction is not a fundamental characteristic of conditioning, but a characteristic determined by the special conditions of Pavlov's experiments, and that it is an instance of inhibitory conditioning, hence of the conditioned response.

Winsor, in an article in the *Psychological Review* (1930, page 399 ff.) has offered a very interesting and plausible explanation of the form of temporary extinction developed in the Russian experiments using the salivary secretion as the response and food as its stimulus. He points out that the conditioned response is established by ringing the bell and then offering food; and that in temporarily extinguishing the response the method is changed. The bell is rung, but no food follows. This is a situation well calculated to produce irritation and so inhibit secretion. The irritation is cumulative and so extinction is finally achieved. But the passing of the excitement after an interval may account for the reappearance of the conditioned response.

I suggested a much less direct and adequate explanation to the effect that a temporary attitude was established, like that in a balky horse or an obstinate person, the secretion falling off because the primary cause of it, the food or the chewing, was missing, and stray distractions have a cumulative effect in inhibiting the response. But this inhibition is conditioned upon the particular circumstances of this particular sitting, and may not be

revived when the experiment is renewed. The inhibition may depend upon a particular posture maintained throughout a special occasion, just as a fit of obstinacy in a human being may be self-maintained and dependent on the general set of the moment.

If you strike a person on the forehead with a rolled paper, blinking is the normal result. If you continue, blinking at first occasionally fails, and then, as you continue, frequently does not occur. If you now allow a few minutes to elapse, or introduce a sudden diversion such as a slap on the back or a loud noise, blinking will again follow the blow. This suggests that something resembling the attitude which I described has been built up, a set which inhibits blinking, and which is gone after the sudden interruption of the slap on the back or the noise, or after the lapse of a short interval.

Humphrey, in his recent book on *The Nature of Learning in Relation to the Living System*, regards "learning not to do," which he calls *habituation*, and associative inhibition as different phenomena. In his opinion habituation is a primitive characteristic of behavior; the gradual diminution of response to a repeated stimulus is, for him, "an instance of the process whereby a living system re-establishes a conservative mechanical equilibrium with the environment when equilibrium has been disturbed. It is found at all grades of life, and in the higher living forms it may be effected by specialized structures such as the receptors and, apparently, the central nervous system, when it is known as sensory adaptation and negative adaptation respectively. There is no

rule which will determine whether it will take place in a given organism for stimuli of a given intensity, quality, or temporal interval. Teleological considerations do not seem to apply."

One of Humphrey's illustrations of this primitive habituation is taken from an experiment of his own in which land snails were placed on an oaken platform which was jerked at regular intervals of two seconds by an electrical attachment. At first the snails retracted their horns at each jerk of the platform; gradually this response diminished and disappeared. The phenomenon cannot be classed as fatigue, he argues, because several snails retracted their horns only to the first jerk, and because a more intense stimulus restored the response. "A fatigue," he says (1933, page 137), "that is diminished by more intense stimulation of the same kind seems self-contradictory." The contradiction is not at all evident to me, since I am under the impression that this is a characteristic of all fatigue.

By the term "habituation" Humphrey includes the diminution of response that may result from sense-organ adaptation, which Adrian has directly observed, or diminution from a "central" adaptation which Humphrey calls negative adaptation, or diminution from effector fatigue (1933, page 147, note).

The present account differs materially from Humphrey's. We have left no room for the phenomenon which he calls negative adaptation, classifying those instances of diminution of response to repeated stimulation in which rest brings recovery as fatigue, not as

learning. It seems preferable to reserve the term "learning" for the more lasting changes in behavior tendencies. And we have suggested that there is a rule which will determine whether or not a response will follow a given stimulus more certainly or less certainly. The rule is: If the response has followed a given stimulus on one occasion, it will more probably follow that stimulus on the next occasion; if the response has not followed the given stimulus on the first occasion, it is less likely to follow it on the second. The exceptions to this rule we have assumed to be instances of fatigue, or of stimulation within the relative refractory period of a response, or of sense-organ adaptation.

We find it hard to rise in time for breakfast. We buy an alarm clock. Is there any rule by which we can predict whether we will rise more and more promptly under the stimulus of the alarm or will relapse into that too common state of entire disregard of its summons? There is such a rule, and it is this: If, on the first morning, we rise promptly, there is an increased chance that this will happen on the next morning. If on the first morning we hear the bell and reflect that there is time for another minute or two of precious sleep and as a result merely turn over and bury our head deeper in the pillow, this is what is probably going to happen on the second morning. By not answering the alarm we may eventually be able to sleep through it without discomfort. By answering it we insure that it will be answered in the future.

The issue that Humphrey raises is this: Is it necessary to have, in addition to association or conditioning, a de-

scriptive category of habituation, defined as a diminution of response resulting from repeated stimulation? For my own part I believe that the phenomena of diminishing response to repeated stimulation are adequately described by the terms sense-organ adaptation, fatigue, and refractory period, and, besides these, associative inhibition or inhibitory conditioning, which is, unlike the others, not temporary in effect.

My conviction on this point, however, is not so firm that I am unprepared for a shift to Humphrey's opinion on very slight additional evidence. More detailed analysis of the phenomena may demand recognition for a primitive form of learning consisting of a tendency for reaction to repeated stimuli to diminish and disappear. In the experimental work so far reported this phenomenon is transient, and the reaction recovers with lapse of time.

In the two chapters preceding this we have discussed the conditions under which a response becomes attached to a new cue. In the present chapter we have considered the circumstances under which responses become detached from their former cues, and the former cues acquire an actual inhibitory effect on their original response. This has been explained as the attachment of the former cue by association to new behavior which inhibits the original response. Whether the response is prevented by fatigue, by inhibition, or by the fact that the stimuli are below the threshold, the result is the same. The cue becomes a cue for other behavior and is alienated from its response. Temporary extinction has been

explained as depending on the building up of a temporary state such as excitement which inhibits the response but is dependent on certain transient elements in the situation. Humphrey's suggestion that the diminution and disappearance of response to a cue is a primitive form of learning distinct from conditioning has been rejected, on the ground that the phenomena he cites are adequately described by the concepts of fatigue, sense-organ adaptation, and refractory period, which are temporary effects, and from which recovery is a matter of physiological process, and in addition to these inhibitory conditioning, which is merely an aspect of conditioning, namely, the conditioning of an inhibitory response.

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Chapter VI

GENERALIZATION

One very startling feature of the conditioned reflex as it appears in many experiments is the tendency for the reflex to respond to stimuli *which have not previously accompanied it.* In Pavlov's laboratory a tone may be used as the substitute stimulus and after several pairings of tone and food the reflex will be in evidence as a conditioned response to another tone. If a touch on the flank is the practiced signal, then, after some practice, a touch on some other part of the dog's body will occasion the flow of saliva. This tendency for unpracticed stimuli to elicit the reflex Pavlov has referred to as irradiation. It has been also called generalization.

Generalization has a number of characteristics. It appears soon after the beginning of practice and disappears as practice continues. It is usually confined to unpracticed stimuli from the same class of sense organs, although this rule has exceptions. And Pavlov has asserted that when the conditioning employs a touch as the signal, touches on other parts of the dog's skin are effective in proportion to their nearness to the original practiced spot. Touches on distant areas start only a few drops of saliva, while a touch in the immediate neighborhood of

the practiced area is almost as effective as this area itself. The experiments on which this generalization is based are rather sketchily described in Pavlov's *Conditioned Reflexes* and the reliability of the correlation between distance from the original site and effectiveness in eliciting salivary secretion is not determined.

Pavlov believes that this phenomenon is to be explained by the spread of a state or condition in the brain from the projection area of the sense organ, the part of the cortex to which sensory nerves are relayed from the mid-brain. This state is supposed to be a state of excitation spreading from the point corresponding to the sense organ used as the signal to neighboring centers and ultimately to more remote areas. The spreading of this unknown brain state is supposed to stop eventually if practice is continued and the areas affected by it grow again smaller until the reflex is finally elicitable only through the sense organs used as substitute stimuli in practice.

Something of the nature of this generalization undoubtedly occurs in the laboratory and in ordinary experience. Are we to accept Pavlov's view that this generalization is one of the primary characteristics of conditioning, and is to be explained by his speculative and remarkable brain state, or can generalization be accounted for in terms of ordinary conditioning without invoking a new and otherwise unknown physiological process?

Taken at its face value this phenomenon is not conditioning in the sense of associative learning. *The new*

stimuli are effective without ever having accompanied the response.

We may, in the first place, be allowed to express some skepticism concerning Pavlov's explanation in terms of the spread of an excitatory state to neighboring brain areas across the usual pathways of conduction. Pavlov believes that both excitatory and inhibitory states can thus spread in the cerebrum. The spread of excitatory states accounts for generalization and the spread of inhibitory states for temporary extinction, which is the result of applying the signal repeatedly without following this with food. Both these supposed states, excitatory and inhibitory, are like no phenomena yet observed in nervous tissue by the physiologists. They are of no use in the explanation of generalization and temporary extinction because they are beyond any present means of observation.

We have excellent reasons for believing that the conditions determining these two phenomena are not the obscure and speculative brain changes suggested by Pavlov, and that they will be found to depend on certain conditions more open to observation. We may first remark that the substitute stimulus of the laboratory record is probably not the actual conditioner. The tone or the touch is habitually presented in Pavlov's laboratory some two seconds before the food. In two seconds impulses from these sources have had time to travel some eight hundred feet if there were available pathways. It is quite probable that the direct impulses from ear and skin sense organs are finished before the end of the two-

second interval, and that the actual stimuli which accompany the salivary secretion and eating movements are movement-produced stimuli attending on listening to the tone or on shifting position in response to touch. The fact that cue and reflex can be separated by several minutes makes this even more probable.

If these movement-produced stimuli are the actual conditioners, the stimuli actually contiguous in time with the response, the phenomenon of generalization or irradiation is very simply explained. Listening to a different tone may involve listening movements so much like those called out by the practiced cue that these similar movements furnish the real conditioners of the action.

The inverse correlation between the distance of the point stimulated from the point used in practice and the strength of the response becomes, to my notion, an excellent reason for the view of the nature of generalization here presented. Touches on neighboring areas of the animal cause similar protective movements. Touches on distant areas elicit movements that have less in common. Touched on the right flank the dog may withdraw somewhat to the left and turn his head to the right. Some phase of this action may be the actual conditioner of the response, the stimulus combination associated with the response. When the dog is later touched on a spot ten centimeters from this point, the touch stimuli are new. They have not been associated with the response. But the movement resulting will be almost a repetition of the first movement, and may hence serve to call out the response in almost its original strength.

Pavlov himself does not accept this explanation and has offered (1932) a number of reasons. He does not believe that impulses from proprioceptive stimuli ordinarily reach the cerebral cortex or can act as conditioners. How any motor skills or motor habits could be acquired if this were true is very difficult to understand. Furthermore, and this is the more serious objection because it is made by an experimenter much more familiar with actual experiments in conditioning than I am, Pavlov states that the movements of listening or the movements made in response to the substitute stimulus may, in the course of the experiment, disappear. The dog may eventually, when the tone is sounded, go through the motions of eating without showing any evidence of the listening that first occurred.

Pavlov's observations on this point are entitled to much more respect than my own very cursory ones, and only future experiment and observation can decide whether the rôle of movement-produced stimuli is as important as here represented. I remain convinced that it is. Listening movements do normally become less and less conspicuous, unless, of course, the noise is so intense that eating and secretion are inhibited and listening prevails. In that case, the presentation of food will probably become a cue for listening. It is quite possible that this diminution of listening movements accounts for the observed loss of generalization as practice continues. Listening to the practiced tone is gradually inhibited through anticipation of food and listening may be so reduced and *altered* that only the altered response to the

practiced tone, and not the uninhibited response to the unpracticed tone, will serve as a cue for secretion and eating movements.

In this explanation certain familiar events serve instead of the highly supposititious spread and retreat of an excitatory brain state proposed by Pavlov. The fact that listening movements are normally present at the beginning of practice, and that these are altered and diminished as practice goes on is reported by Pavlov himself.

Human behavior has many parallels to this phenomenon of generalization. That recently practiced acts for which the person is still "set" may be released by a wide variety of stimuli was illustrated by Jensen's experiment showing the resumption of nursing when an infant is stimulated in almost any sudden manner. Practice narrows the field of eliciting stimuli to the regular attendants of the movement. The infant ceases to accept any nurse and responds only to its mother or its regular nurse. Puppies lose their general friendliness and become devoted to their masters, unless they have been made neighborhood pets. The child becomes shy in the presence of strangers unless hotel life has accustomed him to a variety of persons. The young man finds himself interested in one girl to the exclusion of others unless a number of previous shifts of attention induced by external circumstance has prevented such fixation. Pavlov's dogs fed exclusively on milk later showed no interest in meat. Probably only mild starvation would develop such an interest. As infants we all attempted to be omnivorous, but sand and soap have lost their attraction and we have

all moved toward an addiction to our national dishes, unless travel has practiced us on variety. In each of these cases certain acts could be, soon after they were begun, occasioned by a large variety of situations, but they are after long practice gradually limited to the situations which have regularly accompanied them. Our behavior becomes increasingly specific in its adjustment to recurring stimulus patterns. A child rides on broomsticks, chairs, the banister rail, his father's back, or the St. Bernard. When he grows up he has specific forms of behavior for all of these and riding is confined to the range of his practice.

At first we give the same response to similar stimuli. A child frightened by an animal will show distress on being confronted with other animals of different shape, or even with bits of fur, or cloth with a long nap. These generalized stimuli are not the same that accompanied the first fright, but they have some common feature to which we have given an identical response, or a response identical in some respect. The movement-produced stimuli of the response serve as the conditioners for the generalized behavior. Out of many experiences with the family dog the child eventually has a perceptual response to the bark, the footsteps, the sight of the dog in many positions and distances because these have been present when the perceptual behavior was in progress. When this has been achieved, one unfortunate experience with the dog resulting in fright will be found to be generalized. The child is frightened at the sound of the footsteps, at the bark of the dog when it is out of sight, at

any of the visual patterns which have previously been associated with the perception. Very few of these have been directly conditioned or associated with the fright, but they are all able to evoke it. They have the power to evoke fright because they evoke the perception of the dog, which has become a signal for fright. In Pavlov's dog listening was conditioned to salivary secretion. Any response, then, that provokes listening will occasion secretion. The nature of the stimulus by which listening is called out is at first unimportant. But with the repetition of the experiment, listening is altered by eating and eventually only the stimulus which has regularly preceded that particular, modified form of listening will be effective. The bell has been made the specific stimulus for eating because a specific response to the bell has been developed by the practice. Only the bell has preceded this modified response, not the other sounds.

A dish eaten on shipboard while on the verge of seasickness has a strong flavor of orange. Weeks later the sight of an orange brings on a slight qualm and a memory of the ship's dining saloon. The sight of an orange has not been associated with qualms and has no direct connection with its present response. The connection lies in the fact that the sight of the orange has often accompanied the taste of an orange, and the fore-taste called up by the orange is a perceptual response which undoubtedly includes behavior and stimulation from this behavior. But this perceptual response has been present on shipboard as a consequence of the taste. It has become a cue for the associated distress.

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Chapter VII

THE EFFECTS OF REPETITION

One of the oldest laws of association has been the law that the strength of an association depends directly on the frequency with which it has occurred. Aristotle speaks of "the rapidity with which we recollect what we frequently think about" (452a). Aristotle had also noted exceptions. "But," he says, "it is a fact that there are some movements, by a single experience of which persons take the impress of custom more deeply than they do by experiencing others many times; hence upon seeing some things but once we remember them better than others which we may have seen frequently" (451b). The rule and its exceptions have puzzled psychologists ever since.

It is an undeniable fact that frequency of connection of substitute stimulus and response or of memory and cue normally influences the associative strength or the degree of probability that the response will follow its signal. Pavlov says in a recent article (1932), "During our first experiments often fifty to one hundred or more repetitions of the procedure were required in order to develop a complete conditioned reflex, but now ten to twenty times are sufficient, and often much fewer." Eb-

binghaus found that the memorizing of series of nonsense syllables tested the next day by the amount of time necessary to relearn (the so-called "savings method") showed associative strength directly proportional to the number of times the series was repeated. A mass of laboratory evidence as well as common experience would seem to indicate that the more often a response has followed a stimulus cue the more apt it is to follow that cue. Aristotle speaks of words, tunes, or sayings, which become inveterate on the lips. "People give them up and resolve to avoid them; yet again and again they find themselves humming the forbidden air or using the prohibited word" (453a). Our habits become notoriously predictable with exercise. Habits of long standing are beyond our control, by which is meant that they are so automatic and predictable that they march on in spite of social deterrents, advice, or good resolutions. We are the slaves to a habit when the habit takes precedence over our other habits of polite behavior or morals, and such slavery seems obviously the result of long usage.

Close examination, however, makes the law of frequency less certainly true. We have first those instances in which a habit is established with one repetition, a name recalled with one introduction, a terror founded on one incident, a childhood memory restored in middle age without intervening practice. These are admittedly exceptions to a rule, but a proper rule will provide for its exceptions. To cover such exceptions another law has been formulated, the law of "vividness" or of "intensity." It is explained that, other things being equal, of two as-

sociations with the same cue, that one will prevail which was formed under circumstances that made the experience more vivid, or under circumstances which included greater excitement. In the latter form this law will require separate examination and will be the subject of a separate chapter.

Another class of exceptions is furnished by instances in which a habit of many years is supplanted by one just acquired. We may have on thousands of occasions raised our hat to our female acquaintances, reached on the left side for the emergency brake, made a certain turn on the way from office to home, waked at seven-thirty, until these acts are stereotyped and dependable. If we don a straw hat we find ourselves clawing vainly at its flat crown when we try to remove it; or on buying another car, grasping at thin air for a brake which is on the other side; or taking the old turn after we have changed our residence, or still waking at seven-thirty though we need not rise until eight. But it seldom requires for the new adjustment practice equivalent to the practice of the discarded act. Very often a few awkward blunderings only, and we have changed a habit of years. If the law of frequency were taken literally it would take years to undo the habits of years. This is obviously not so.

Another qualification of the law of frequency is made to cover such cases. This is the law of "recency." Of two associations which have had equal practice, the more recent will prevail. This law we shall examine here and now.

It has been the custom in text-books of psychology to state that the law of frequency is based on the physiological events at the synapse. Conditioning clearly implies a change in the course of nervous impulses through the nervous system. It has been assumed that impulses are "drained" into active motor pathways, and that each passage of an impulse over a new path so established "weakens the resistance" of that path and so makes it more certain that the next occasion will find the impulse going that way. This has been held to be a fundamental characteristic of conditioning. The more times the cue occurs with the response, the more certain is the cue to bring about the response. Pathways are "worn" with use and become easier to traverse.

This account of the basis of the law of frequency has been so taken for granted that it is quite astonishing to find that there are serious reasons for doubting its accuracy. Peterson called attention to this (1922), having found that with human subjects learning the "mental maze" the effects of recency and of frequency could be negative instead of positive. Many other experiments have thrown doubt on the generality of the law of frequency. And there is no direct physiological evidence that pathways through the nervous system are thus established by "wear." The resistance at the synapse or functional connection between successive neurones cannot be directly observed so that explanation of the law of frequency in terms of such resistance is pure speculation. If recent experiments tending to demonstrate associative learning in animals without a synaptic nervous system are

confirmed, the synapse will still less be able to support the burden of explaining learning. So far as the facts are concerned it is equally probable that conditioning is an all-or-nothing affair, like the setting of a switch rather than like the wearing of a path. In other words, it may be that the law of recency describes one of the fundamental characteristics of associative learning and that the results of frequent repetition depend on the enlistment of more conditioners, not on the strengthening of the association of any single conditioner.

I believe that the hypothesis that conditioning involves a definite rerouting of impulses from sense organs, and that this rerouting persists until further conditioning alters it is more sound than the hypothesis that associative strength varies directly as the number of pairings. It can be reasonably argued that of two responses associated with a given substitute stimulus the second is always the one conditioned by that stimulus. The second will not always follow its cue, obviously, because other cues may elicit the first. But if any cue has been acting at the time of a response different from the associated response, that cue will become a conditioner of the new response, and whatever effect it has will be to further the new response. If it is not immediately effective in bringing about the new response, it will again serve to energize whatever response is prevailing and will be again alienated as a conditioner.

If this is the case there is much to be explained. The facts seem directly to contradict this view. Practice of a response with a cue has generally an increasing effect

on associative strength. How can this undeniable fact be reconciled with the view that practice of a response with a cue does not have an increasing effect on associative strength?

Some light on the nature of the answer is thrown by Pavlov's remarks quoted at the beginning of this chapter. He said: "During our first experiments often fifty to one hundred repetitions of the procedure were required in order to develop a complete conditioned reflex, but now ten to twenty times are sufficient, and often much fewer." What change has taken place in the methods of the Leningrad Laboratory which would make less practice necessary? It is my belief that the improvement has been in the control of the conditions under which the experiment was taking place. There has been achieved more uniformity of procedure. More practice was formerly necessary because, in spite of the sound-proof experimental room, the removal of the experimenter from the room in which the dog is placed for the experiment, in spite of all these precautions a great part of the stimuli affecting the dog are not controlled successfully. Standing in the loose harness the dog can shift his weight from one leg to another, turn his head, prick up his ears, yawn, stretch, in fact alter his whole pattern of proprioceptive stimulation, and a certain amount of his exteroceptive situation. It is my belief that if all these features of the dog's situation could be made uniform from day to day, not fifty or one hundred, not even ten or twenty practice periods would be necessary for establishing the certainty of conditioning, but only one.

The dog is responding not only to the food and to the bell, not only to his state of hunger with its attendant stomach contractions and general muscular tension, but to his own posture, to his own movements through the movement-produced stimuli in which they result. These are subject to variation. Improved laboratory practice reduces that variation. Pavlov mentions that some preliminary practice is necessary to accustom the dog to the apparatus before starting experimentation. The need for this practice is to eliminate the restless movements by which the dog would so vary his situation that conditioning would take more time.

On the first occasion on which the substitute stimulus is presented the dog may be found responding to some minor skin stimulation, turning his head to the right, easing muscular fatigue by shifting his position. These bring movement-produced stimuli which accompany his response to the food in a much truer sense than the substitute stimulus which occupies the experimenter's attention and record. The bell, if it is used as the substitute stimulus, may have ceased to ring many seconds before, so that only the movement-produced disturbance which it has left remains to serve as the conditioner of the response.

In Pavlov's own laboratory it has been established that substitute stimuli have under some circumstances an additive effect. A response conditioned upon two separate stimuli on separate occasions will be stronger and more certain if the two conditioners are presented together. Two conditioners generally are more effective than one,

just as a conditioner presented with a conditioned inhibitor results in interference and diminished effect. On the first practice period not only the bell and its direct motor effects but other accidental features of posture have all become conditioners. If they could all be reproduced together, the one practice period would very probably turn out to be sufficient. It is not ordinarily sufficient because there are uncontrollable differences in the situation on the second presentation of the substitute stimulus.

But why should practice make the effect increasingly certain? Is it not quite possible that on successive practice periods more and more conditioners are enlisted, so that after twenty periods there is a high probability that the cue will have enough support to be effective? Pavlov has remarked that any minor disturbance, like that made by a fly in the experimental chamber, will ordinarily prevent the response from appearing. The most reasonable supposition is that this fly has so changed the situation that the supporting cues from movement-produced stimuli are dissipated. The bell has rung, but the usual movements of listening have been disrupted by the fly. No saliva flows because the cues are lacking. If flies were always introduced into the experiment a conditioned response to the bell might eventually be established, but it would probably require several hundred practice periods and not even then reach the point of dependability usually attained.

"Practice makes perfect." We do not take this adage too seriously because we know too many exceptions,

but there is some truth in it. We should probably say, if we are to be exact, not that practice makes perfect but that perfection is seldom approached without practice. Many writers have confused this fact with the facts covered by the law of frequency or use. They are, however, quite distinct. Perfection, or progress in the direction of perfection, is not at all the same thing as increased certainty of response to a cue.

The attainment of perfection demands that awkward and useless movements be detached from their cues as well as that useful movements be attached to cues. The increase in skill that so commonly results from practice is then no evidence for a fundamental law of frequency in describing the nature of the conditioned response. How skills are attained must be left for another chapter, since it is a very distinct problem.

Our proposed explanation of the law of frequency and its exceptions is then as follows: The law of frequency is not a fundamental characteristic of conditioning. The observed effects of frequent repetition are not to be explained in terms of increased associative strength with added repetitions, but in terms of the enlistment of added conditioners which is normally the result of repetition. The exceptions to the law of frequency are not exceptions to the general principle of conditioning, but illustrations of that principle. Those exceptions which have been classed under the law of "intensity" can be explained in terms of the nature of motor excitement, which will be done in another chapter. Those exceptions which have been classed under the law of "re-

cency" are merely illustrations of simple conditioning, in which the last conditioning probably always prevails.

The reason that one occasion is not enough to rid us of an annoying habit, though the cue is present and we have succeeded in inhibiting the response, is that not all the cues were present, and not all the possible conditioners were alienated. On each successful inhibition some of these cues may be attached to the inhibiting response and eventually we may have enlisted such a proportion of cues for our inhibiting behavior that the annoying habit will appear only occasionally, when some one of the more rare stimulus patterns which have not been reconditioned happens to be present. The habit of smoking is in reality made up of thousands of habits. The sight of tobacco, the smell of it, the mention of it, the finishing of a meal, finishing an office task, looking at the clock, and innumerable other situations have become all signals for smoking. We resolve to stop smoking. We substitute for a few of our conditioned responses inhibitory responses, a grim closing of the mouth, a tendency to push away the pipe, groping movements ending in substituting chewing gum, or nails, or pencil. We suddenly find ourselves smoking. Some cue which we had not alienated has taken us unawares and had its usual response. As a result we find it necessary to practice not-smoking on numerous occasions before our rejection is a settled habit. The apparent exceptions to the rule that the most recently practiced stimulus-response sequence will prevail, turn out not to be exceptions in fact.

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Chapter VIII

EMOTIONAL REINFORCEMENT

THE most startling exceptions to the rule of frequency are those incidents in the experience of all of us in which a memory or a motor habit is so impressed with one occurrence that it is beyond our control. Once the cue is encountered, the response is inevitable,—as inevitable, to use Aristotle's simile, as the course of a thrown ball that has left the hand. The proper appreciation of the rôle that such events play in human life was left to Freud and the psychoanalysts to uncover. The psychoanalytic literature expresses a lofty disregard of the law of practice and leaves this to the academic psychologists as a harmless laboratory subject. The psychoanalyst is interested in those experiences which rise above practice and impose patterns on behavior which alter lives and fortunes without need for humdrum repetition. Our memories for names are not explained in terms of the number of rehearsals we give them, but in terms of the wish to remember or the wish to forget. Perverse tendencies in behavior are not the result of habit but of one tense experience. Tics, those maverick habits which distress us because they are actions out of place, may, it is true, be somewhat harder to eradicate if they have long standing,

but they are said to be established beyond control because they attended some strong repression, and they are said to be removable by one experience, the consciousness of their origin.

Suppressed behavior does not, in the Freudian accounts, lead to inhibitory conditioning as our present theory would lead us to expect. It continues to affect behavior in the form of unconscious wishes and repressed desires.

Every person's experience holds instances of fears based on one terrifying event, of strong distastes founded on one incident, of embarrassment dating from one unfortunate encounter, of names remembered with one saying.

Under what circumstances does this super-learning occur? Why will one telling experience sometimes take the place of twenty practice periods? The psychoanalysts have suggested the answer and there is reason to believe that it is substantially right. All these cases of phenomenal learning seem to take place under strong emotion. The behavior that is attended by strong emotion seems to undergo fixation. What leads to excitement tends to become a strong interest. The acts we perform when depressed or relaxed leave less impression.

If exciting emotion is the common circumstance that explains our instances of accelerated learning, how is this accomplished? Laboratory experiment has offered some faint evidence that there may be direct effects on the nervous system of the hormones known to be or those suspected to be in the blood stream in increased

quantity in exciting emotion. Strychnine and caffeine seem to have such effects. But it has not been established that the accelerated learning brought about by substances in the blood stream is to be explained by their action on nervous tissue. There is another possible source of their effect. In those drugs which exaggerate muscular response the acceleration of learning may be dependent on the extended action on sense organs of muscles and tendons resulting from the increased amplitude of muscular response. Exciting emotion involves the quick relief of fatigue through the direct action of adrenine on muscle tissue. The muscle so affected responds with greater contraction to the same motor impulses and by its greater contraction stimulates new fields of proprioceptor sense organs in the muscle and the joints and tendons. These added stimuli may become conditioners of movement and hence explain the increased certainty of conditioning. Movements during excitement are more complete and vigorous than movements during unexcited states. In excited movement there are new movements to be conditioned as well as added conditioners for movement. The strong tendency for movements executed during excitement to become stereotyped into habits may have this explanation.

Excitement is not the result merely of endocrine action. There are contributing factors in excitement which are better established than are the effects of any endocrine action with the possible exception of adrenal secretion. Muscles when they contract stimulate their own sense organs and the resulting impulses are respon-

sible for an additional contraction of the muscle itself, as well as contraction of allied muscles. Not only contraction, but stretching, and sudden resistance to contraction have this effect of addition to the original tension of the muscle. The contraction of any muscle thus leads to a sort of reverberation which tends to increase muscular tonus. The main feature of states of excitement is undoubtedly this general increase of muscle tonus which tends to be self-sustaining through the circular reflexes described, and to exaggerate or reinforce whatever action is taking place. There is also evidence reported by Cannon that muscular contraction tends to accumulate general muscular tension through endocrine secretion in the muscle. Cannon has called the presumptive secretion of the tense muscle secretin.

Through muscle-to-muscle pathways action and posture tend to be maintained. Even the knee-jerk is a somewhat prolonged tetanic contraction and in no wise a muscle twitch, though it is elicited by a single stimulation.

States of excitement thus tend to maintain themselves or to increase if there is intense stimulation present. They also tend to increase if there is recurrent stimulation which results in an accumulation of tension in a muscle. Sherman and Sherman report (1929) that high muscular tension and crying could be produced in infants either by intense stimulation for a short time, or by very mild stimulation repeated at short intervals. For instance, if the edge of a card is drawn lightly along the sole of the infant's foot a Babinsky reflex is elicited along with some

general activity. When this is done a second time, before the effects of the first stimulation have worn off, there is evident more activity. After five or six repetitions the infant will be crying and in a state of general excitement. General tension can be built up not only by intense stimulation and by recurrent stimuli with resultant accumulation of tension, but also by resistance to the action of muscles. Obstacles to action result in building up states of excitement which have an obvious utility.

It has been suggested by Holt in his *Animal Drive and the Learning Process* that these muscle-to-muscle reflexes are themselves the result of conditioning. If the muscle contracts, the contraction is necessarily accompanied by the stimulation of sense organs in the muscle itself. If such pathways are subject to conditioning, muscular contraction would tend to be self-sustaining, or lead to a progression of movements.

Understood as a general condition of increased muscular tension leading to exaggerated action, states of excitement would lend themselves readily to accelerated learning. There is some evidence that such simple tasks as learning a series of nonsense syllables are facilitated by the slight addition to muscular tonus provided by gripping a dynamometer while practicing (Bills, 1927). The improvement in the speed at which one can add figures is much more rapid under the same conditions. Congo schoolmasters speed up the acquisition of associations in their drowsy and relaxed pupils by making them study while standing on one leg. Drowsy husbands at evening lectures remember more of the discourse when occasional

conjugal nudgings maintain a state of respectable muscle tonus. At the symphony concert they can often recall only what followed the fortissimo passages. Reclining chairs in the lecture hall would be fatal to the spread of information.

An interesting feature of the emotional reinforcement of memory has been pointed out by Stratton. While his evidence is anecdotal, the experiences are common to all. Not only the events which accompany excitement are subject to unusual fixation in memory or hypermnesia, but the events which *precede* the excitement may be likewise so fixed. The reason for this is probably that such events are recalled or rehearsed during the excitement and so share the reinforcement. We recall what we were doing on the morning of the day of the fire, though the fire took place in the afternoon. We have forgotten what we were doing the morning before or the morning after. The associations tentatively established in the morning would have faded by now if they had not recurred during the excitement of the afternoon and so been fixed.

In his *Progressive Relaxation* Jacobson has reported a number of observations which are very interesting in this connection. When his subjects were by long training taught to relax one muscle group after another, and eventually muscles of the neck and the external muscles of the eyes (which last could be verified by the absence of the involuntary movements under the lids) they reported that they could not evoke any visual imagery on request until they had developed noticeable tension in the eye muscles. Nor could they "think" a sentence

without first developing noticeable tension of the muscles used in speech. Many thought trains are obviously accompanied by appropriate slight movements. It is my belief, though certainly it is not an established fact, that all thought trains depend on movement trains, and that in all instances the association of ideas is an association of successive movements through conditioning. St. Augustine (*De divin. daemon.*) was of this opinion for he explained the fact that the devil, though he was not omniscient, could so well read human intentions and thoughts by the fact that all thoughts are accompanied by slight appropriate movements and the devil's skill was in the recognition of these movements. That this was what Aristotle meant when he wrote the following passage (453a) is not impossible. "That the affection (memory) is corporeal, i.e. that recollection is a searching for an 'image' in a corporeal substrate, is proved by the fact that in some persons, when, despite the most strenuous application of thought, they have been unable to recollect, it (viz. the effort at recollection) excites a feeling of discomfort, which, even though they abandon the effort at recollection, persists in them none the less. . . ."

Certainly in states of excitement, thought is more apt to be accompanied by obvious action, and the reinforcing effect of excitement could clearly be the result of the great increase of movement-produced stimuli available as substitute stimuli.

Not only is excitement, in the form of increased tonus and consequent increased action, an accelerator of learning, it is itself subject to conditioning. Stimuli present

during excitement on later occasions have become conditioners of the total behavior pattern, and, in addition, each part of the behavior pattern tends to condition the rest, and an integrated and stereotyped response may be developed, sometimes of extreme complexity. Hysterical seizures are commonly rehearsals of behavior which included an emotional crisis, a hostile attack, an erotic incident, a fit of rage, or a terrifying accident. The seizure re-enacts the original event. Psychoanalysts make a point of seeking for this re-living of an emotional experience as a step in its cure, though a mere re-enactment is of very doubtful value. The hysterical attack is subject to increased fixation through practice.

That overt action leads more readily to the stereotyping of a serial response has confirmation in experiments showing that material read aloud or material recited is more quickly learned than material read silently. Barlow, for example (1928), found that lists of words which were whispered as read were better retained than when read with a pencil held in the teeth.

The importance of conflict in human behavior has been forced on the attention of academic psychologists by the psychoanalytic movement. The essential nature of any conflict is the innervation of opposed action systems, resulting in an increased muscular tonus and the other symptoms of excitement. A situation which includes conditioners for two opposed actions results in the normal individual in excitement and a consequent unstable equilibrium. One system gets under way and inhibits the other. In great fatigue, or in illness, or for

other causes, the requisite emotional reinforcement may be lacking and the result be a chronic low tension insufficient to resolve the conflict. Many neurotics have learned, according to Janet, that quarrels, risk taking, even the added excitement of committing a theft will bring the necessary emotional reinforcement and resolve the conflict. Behind every neurosis there is a difficult choice, with an attendant tension and consequent physical exhaustion. If the outstanding symptom is the difficulty of choice it is customary to call the neurosis psychasthenia; if the symptom that attracts attention is the exhaustion, the name applied to it is neurasthenia; if the conspicuous feature is the chronic (but inadequate) excitement, the disorder is referred to as anxiety neurosis. The situation usually starts a vicious circle; the unresolved conflict or choice results in a state of exhaustion which makes its resolution still less probable. The primary occasion may be a physical lack that makes choices difficult, or it may be a conflict that brings in its train physical inadequacy.

The reinforcing effect of excitement on learning is of immense importance in determining the development of human interests and controlling the nature of human values. Until we are of an age when the physiological reserves of muscular activity are diminished and excitement is difficult to produce we tend to fix our habits about those activities which induce excitement. Dangerous sports, risks, war, play (which can be defined as activity which furnishes its own reinforcement), noise, bustle, all are interesting and compelling because they

have a tremendous advantage in forming habits, in establishing conditioning. As we grow older, or become ill, excitement is followed by exhaustion and the activities suffer conditioned inhibition. Sport loses its fascination; we enlist for office duty instead of in the regiment of the line. We no longer sympathize with our children's passion for bedlam.

The young person in good health learns to do what is exciting. He does this because excited action is more readily fixed as habits. It is more energetic and more extensive. In old age and in illness excitement is avoided because the energetic action brings exhaustion, and *the activity ceases while its cues remain.* Inhibitory conditioning occurs. The sights and sounds which in our earlier years stirred us to active response have been present when we failed to respond and have so lost their power to thrill.

Jung has pointed out that our intense interests, our violent loves and hates, our strong moral indignations, all betray traces of their opposites. Violent love is more often changed to violent hate than to a state of indifference. The sin which we condemn most passionately is the one to which we are most apt to succumb. This is a very shrewd observation, and Jung's explanation of it is extremely plausible. Violent emotion appears in us only when we are in a state of conflict. Jung interprets such states of conflict in mental terms, but they can be more clearly understood when we realize that they are states of actual muscular conflict. Only if our habits strongly incline us to the sin we condemn, only if the actual be-

ginnings of the condemned act are present in the form of muscular tensions, and restraint and self-control are also present in the form of tensions of those muscles used in avoiding or withholding, can a high degree of emotional excitement be produced. Muscles in opposition are the primary condition of emotional tension.

In like manner our strong interests and passions are dependent on obstacles, which may be external, or the obstacle may be a conflicting habit and so internal. If we mean by "true love" a state of great excitement, its course never could run smooth because with smooth running and without obstacles or conflicting habits, no excitement can appear.

The utility of physiological excitement and of emotional reinforcement of action lies not only in the additional energy of movement which may carry us past the obstacle, but in the effect of excitement on learning. This is twofold. Excitement facilitates learning and the stereotyping of habit and the conflict which is responsible for the excitement breaks up old habits. The outcome is often an improved adjustment in the form of a new habit routine adequate to the situation.

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Chapter IX

FORGETTING

NOTHING seems more sure than that time erases the effects of learning. The events of the last few minutes are always clearer in memory than the events of yesterday, or of a year ago. Almost always, that is. We can recall the last bridge hand, but not for very long. The mere lapse of time seems to soften grief or hatred and to rob us of our skills and accomplishments. The sophomore engineering student finds on entering college in the fall that the mathematics he was able to use in the spring has deserted him, and that the names of some of his companions must be relearned. In the courtroom a detailed account of a long past event is looked on with proper suspicion that it has been "refreshed."

Even the law of recency, which holds that of two associations with the same cue the more recent will prevail, seems to state only this same fact, that the older an association is, the less associative strength it has. But this is not what the law of recency means at all if we examine it closely. It does not assert that the mere lapse of time will have any weakening effect on learning. It states that when two different responses have been attached to the same substitute stimulus or cue, the one attached last

has the advantage over the other. This makes no reference to elapsed time.

Most psychologists still assume that time, just by virtue of its lapse, accounts for the gradual loss of the effects of learning. It is supposed that the metabolism of nerve cells gradually eradicates the structural changes left in learning. It was on this assumption that Ebbinghaus (1913) made his studies of the rate of loss of memory of nonsense-syllable series and established his curve of forgetting. He practiced a series of given length until it could be repeated once from memory without error, but found that a few minutes afterwards it was impossible to duplicate the performance. But not all the effects of the learning had disappeared. The amount left he undertook to measure by finding how many repetitions were necessary to regain his ability to reproduce once perfectly from memory. Trying this with different series he found that the loss was very rapid at first, less and less rapid as time went on, and that after the lapse of a day forgetting was proceeding at a very slow rate.

After Ebbinghaus the investigation of the "curve of forgetting" became a very popular pastime with psychologists, and many studies have aimed at finding for it a mathematical formula. One of the first eccentricities discovered in the curve was a tendency for it to rise somewhat just twenty-four hours after the practice. This was not evident in all the studies, but in a substantial number. It was also to be noted that the curves of forgetting for nonsense material and for sensible, or somewhat sensible, material such as doggerel verse, were

materially different. A sentence of twenty syllables might be remembered for many days after one rehearsal, while the same interval would reduce the ability to recall nonsense syllables almost to the zero point. Strange effects were sometimes evident. Children and prisoners in solitary confinement, for instance, were able to recall better the series they had learned after twenty-four hours than they were after one hour. This phenomenon was given a name, reminiscence, and a place in some of the text-books, and given the far-fetched explanation that the cell changes which must occur in learning continued active some time after the associations were established.

Freudian psychologists have all along paid scant attention to the rôle of time in forgetting, or denied it altogether, holding that nothing is ever forgotten, that memories may be for a time submerged into the "unconscious" but from there may be retrieved intact under the proper circumstances. Of late other psychologists have shown some signs of a changed opinion on forgetting. It is very interesting to note that the Russians, following Pavlov in the investigation of the conditioned response, say very little about forgetting and the effects of the lapse of time on the strength of a conditioned response. They have confined themselves to mentioning that after a conditioned response is established—and by this they mean that it will occur some indefinite number of times in succession, the response has been found present after a lapse of months, or, in some cases, years. Nothing like the definite "curve of forgetting" appears with the conditioned reflex (cf. Razran, 1933).

Attempts to express forgetting as a function of time, to predict the amount of forgetting in terms of the lapse of time, have not discovered any general law. It is true that for some cases of learning, as, for instance, the memorizing of a list of nonsense syllables in the laboratory, a fairly definite relation between time and extent of forgetting seems to hold. The "curves of forgetting" of different investigators have similar shapes. But if poetry, or a skill, or a conditioned reflex is substituted for the nonsense syllable list, the curves of forgetting turn out to have very little family resemblance. The reason for these conflicting results is that the effect of lapse of time on memory depends on what happens during the lapse of time. The evidence from common sense and from scattered reports of experiments in conditioning is that under some circumstances forgetting does not take place. Our question is: What are these circumstances under which conditioning remains unimpaired?

A suggestion of the answer is to be found in the experiments on conditioning. Here ten to fifty pairings of food and a bell in the experimental chamber establish a conditioned response to the bell. Once established this response is stable and is often found in full strength months later, unless the dog has been subjected to some specific process of unconditioning with the use of the bell. The most probable explanation for the stability of the conditioned response and its freedom from forgetting is that learning does not disappear as the result of a mere lapse of time, but only when that lapse of time includes new learning which erases the old. In the case of the dog

in the laboratory the conditioned response has one great protection against forgetting, which is the fact that the conditioned or substitute stimulus is the sort of thing the dog does not encounter in his daily routine. It is reserved for the laboratory situation. Between experiments the dog is exposed to very few of the stimuli that make up the experimental situation. If bells rang in the kennels not followed by the feeding, negative adaptation would surely occur. Such negative adaptation would be forgetting.

This striking tendency for certain laboratory conditioned responses to be apparently exempt from forgetting has been used by a number of writers to argue that conditioned reflexes differ fundamentally from other forms of learning. This is not the case. Any response whose cues have not been alienated in intervening experience will be so preserved and protected from forgetting. The re-appearance of certain childhood habits and childhood skills may occur in old age, if these have not been disorganized through re-learning.

Laboratory studies of the effects of intervening activities on forgetting have used the terms associative inhibition to describe the difficulty of forming an association between A and X because of an earlier association between A and B (Mueller's law), and *retroactive inhibition* to describe the complementary phenomenon, the loss of an association caused by intervening activities. The discussion of retroactive inhibition has turned about the question: Is retroactive inhibition the result of engaging in a similar activity during the interval? When a

list of nonsense syllables has been learned it is better retained at the end of an hour or so when the hour has been spent in some quite unrelated activity than when it has been spent in memorizing another list of nonsense materials (cf. McGeoch, 1929, 1930, and Robinson, 1927). The experimental results have been somewhat conflicting because "similarity" of intervening activity has not been defined. Some similar intervening activities favor recall; some impede it. The probable explanation is that an activity in which some of the cues of the previous learning occur but which are followed by different responses, results in the alienation of these cues and hence in interference; while if the common cues have in the intervening activity been followed by the same responses there is facilitation rather than inhibition. Because of the failure to distinguish these cases, very little enlightenment has resulted from such experiments.

It is very interesting to find that in general a period spent in sleep results in much less forgetting than an equal period spent in waking. Dahl found (1928) that both figures and nonsense syllables were better recognized after six to eight hours' sleep than after a waking period of the same length. After one or two hours of sleep, it is true, less was remembered than after one or two hours of waking, but this has a possible explanation in the circumstances of being wakened after so short a period. Van Ormer (1932) used the savings method in which the practice necessary to re-learn was measured. He found no difference in effect on amount retained between one-hour periods of sleep or waking, a slight

difference for two-hour periods, and a very reliable difference in favor of sleep for eight-hour periods. This is a substantial confirmation of the original experiment by Jenkins and Dallenbach, who found (1924) twice as many syllables retained after a period of one, two, four, or eight hours in sleep as after an equal period waking. Moreover they found the amount retained after eight hours practically as large as after two hours. Of the nature of forgetting Jenkins and Dallenbach say (1924, page 612): "The results of our study as a whole indicate that forgetting is not so much a matter of the decay of old impressions and associations as it is a matter of interference, inhibition, or obliteration of the old by the new." Van Ormer likewise says (page 45): "It is suggested that our results in favor of sleep are brought about by the absence of the inhibition and obliteration of the learned material by the waking activity." McGeoch has very ably marshalled the evidence (1932) against the dependence of forgetting on lapse of time in a recent article in the *Psychological Review*. Common experience verifies this. We recall on waking much more detail of the hour just before sleep than we can in the evening recall of the events of a morning hour, provided, of course, that no extraordinary conditions prevailed on either occasion (cf. Spight, 1928).

If we deny that forgetting is the result of the mere lapse of time, how is the rather regular and predictable disappearance of memory for a list of nonsense syllables to be explained? The answer is that, unlike the laboratory bell, the nonsense syllables, each of which is a cue

for its successor, do occur in the interval following the laboratory practice. Syllables of the series occur in word combinations and acquire new attachments during the interval. As time elapses, more and more of the conditioned cues of the original learning are alienated from their responses and their new responses tend to break up the continuity of the memorized series.

This is probably the reason why so many studies have shown forgetting to be very rapid at the start of the interval, and increasingly slower as time goes on. When the response occurs it probably enlists many thousands of conditioning cues from among the stimuli acting. More of these cues are eliminated at the beginning because there are more cues to eliminate. Accompanying postures and movements and tensions are, as time goes on, incidental accompaniments of other situations, and so lose their associative connection with the response we have in mind. Furthermore, the evidence from experiment that in nearly all cases some faint traces of learning are present even if months or years have elapsed is very plausibly explained by the supposition that there are generally some cues which have not occurred in the interval and so have not been alienated.

The reason why we can go from winter through spring, summer, and autumn and find the next winter that our skill in skating is almost what it was when we left off ten months before is that the postures and movements of skating simply do not find a place in our domestic routine, or in our summer sport. The associative interconditioning is thus not subject to forgetting be-

cause there has been no occasion for reconditioning and inhibitory conditioning. Rowing an ordinary boat during the summer will do more to make a crew man forget his training than will selling insurance, because many of the cues on which his proficiency depends are, in managing a row-boat, necessarily followed by behavior inappropriate in a shell.

Popular tunes heard daily on the radio or in the restaurant lose their power to please. Played while we are conversing, while we are eating, or while we are casting up the amount of the check, they become conditioners of these activities and active inhibitors of the pleased attention they once caused. In the age of the radio, popular songs have a life of a few weeks as compared with the occasional record of over a century for songs which before the days of easy mechanical reproduction were heard only on occasions when they could be given undivided attention. It is not time that robs beauty of its charm, but preoccupation with other affairs in its presence.

Grief is recalled to a large extent by the objects and scenes which were associated with it. A change of residence and new scenes will not always do away with it because it has become conditioned on our own behavior. But the demands of practical affairs will be effective even if we remain in the same surroundings. Those who are at leisure to enjoy their grief may preserve it indefinitely. Those who are compelled to an active part in living find that forgetting has taken place. The reminders which

conditioned the emotion have become conditioners of other activity.

The home in which a much beloved member of the family has died calls out more poignant memories in the survivor who has lived elsewhere since that event than it does in those who have made it their residence, although those who remained at home may have suffered more than the wayfarer who escaped from the sad reminders of the family's loss. Those who continued to live in the home have had grief more often stirred in the days following the death, but they have become more immune to the home as a reminder of their grief. They have lived in it while occupied with practical affairs and grief has suffered inhibitory conditioning.

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Chapter X

HABITS

THE conditioned response has been spoken of as if it were made up of an isolated, stimulating event and a consequent movement. In reality stimulus and response are abstractions from a continuous flow of change in the world and in the responding person. We select some outstanding feature of the situation, some easily observed or some readily controlled change that affects sense organs, and some arbitrarily limited feature or section of the organism's movement and call these stimulus and response. We learn in terms of conditioning to predict that this response will or will not follow a given stimulus.

Considering all our handicaps, our inability to control the whole stimulus situation and the indefinite character of most responses it is astonishing that we can be as often right in our predictions as we really are. Our anticipatory actions are, of course, much more detailed and much more accurate than our verbal predictions. We learn to be ready for the oncoming behavior of others much more easily than we can predict in words what they will do. Words are so roughly fitted to recurring events, so oblivious of their minor differences, that as psychologists trying to anticipate by verbal formulas we have a great

handicap in competing with ourselves as simple men ready for what is to happen in the behavior of our neighbors. We can pull an oar with a friend at the other oar, shift our position in a boat in time to meet his shift, or reach for the food that he is about to pass, yawn at the anecdote he is about to tell, without being able to put this anticipation into a formula. But science is confined to formulas. Science is the formulation of our anticipations of change. If there is one thing in the behavior of our neighbors we can be certain of, it is that it will often escape our verbal rules and surprise us. The most complete psychology will only reduce somewhat the number of occasions when we shall fail to be prepared for what they do.

Action is the result of stimulation which, in a properly constructed organism, signals a threat to those vital states of equilibrium which Cannon has called homeostasis. These states of internal temperature, water balance, blood salt concentration, acid-alkaline balance in the blood, and many others which are the conditions on which continued life depends are protected by the sense organs and nervous system and effectors. The maintenance of life may be viewed as the maintenance of an indefinite number of these conditions in the body. Sense organs in the body are so placed and so made that they are stimulated by natural events which represent threats to the maintenance of these conditions of bodily equilibrium, and all living activity can be viewed as serving these states.

A single disturbance may be reacted to in such a way

that the threatened upset is avoided. This can occur even in mechanical systems or in creatures which are, like plants, without mind. The difference between the reactions of plants and of creatures with mind is that plants continue to react in the same way to a recurring disturbance, while the characteristic of mind is to change the form of reaction to repeated disturbance.

One of the outstanding characters of the changes that take place in learning is the tendency to form, in answer to a repeated disturbance, a stereotyped response which is adequate to the repeated disturbance and which expends less energy than earlier performances. A recurring problem is solved when a routine and stereotyped habit has been formed which returns the animal to the state of equilibrium from which it has been disturbed. Hamilton (1925) placed a variety of animals in confinement from which they could escape only through one of four doors —but which door (excluding the one just used) would on any occasion allow escape was settled by throwing dice. Some of the animals tended under these circumstances to "go to pieces"—to have a "nervous breakdown." Some of the animals, especially apes and humans, formed eventually a routine habit of exploring the doors in a set order and so avoided breakdown. Anrep's experimental neurasthenia in dogs was established in much the same way. By confronting hungry dogs with two doors, one marked by a circle and leading to food, and the other marked by an ellipse and leading to punishment, he found that the dogs quickly learned to choose the door with the circle and to disregard the ellipse. But when the

ellipse was made so nearly circular that the dogs could no longer discriminate between the figures and the cue was now ambiguous, the routine habit was broken up and the essentials of a nervous breakdown were present. Even the ability to respond correctly to the original choice disappeared.

It is probable that the essential cause of every nervous breakdown in human beings is a failure to establish a routine habit response to a recurring situation. What is the process by which these necessary stereotyped habits are established? Under what circumstances do recurring disturbances tend to be answered with a routine and stereotyped reaction?

We can account for the strong tendency to repeat action patterns as they have previously occurred in terms of conditioning. When a series of movements has once taken place it tends to be fixed without the repetition of all the series of stimuli responsible for it in the first place. The reason for this is that movement-produced stimuli have become conditioners of the succession of movements. The serial response is now somewhat independent of its original causes. Once started it may maintain itself, particularly if excitement has made movement and movement-produced stimuli more intense or energetic and more extended.

We can observe in all behavior this tendency to "set" in integrated patterns. Not only the pattern of our signature but our walk, our laughter, our styles of play and work, our conversation, our very trains of thought betray this fixation. We repeat verbatim remarks we have

made before. We cling for years to mannerisms and actions that serve no purpose and only occasionally are noticed by us.

Why should habit series not be stereotyped with one rehearsal? The answer is that an astonishing number of them are. Many a speaker preserves for years some button-twirling movement, some unintelligible gesture, some grimace that was an accidental feature of his first public appearance, or some set form of words that "signs" his address without his knowledge. The first performance of many tasks sets their style and manner for later performances.

If the later situation could be made a complete repetition of the earlier and if there were no intervening learning, serial responses would be duplicated on the second occasion. But these conditions cannot be met. There is always behavior in the interval and some of the cues for movement have been attached to other movements. The situation is always somewhat different. The room and its contents may be substantially the same and the external events duplicated, but the individual is not caught in exactly the same posture or movement pattern.

We shall therefore never find perfect repetition of a movement, in spite of the conditioning of one movement on the stimuli produced by the last. Through this conditioning on movement-produced stimuli, behavior is adjusted to the more significant features of the environment and exertion tends to be minimized. The first acquisition of a new dance step consists in tentative movements which are directed by eye or the instructor's voice. On

repetition, one movement becomes the substitute cue for the next and the series is partially independent of eye and voice. Learning to read music is learning to play phrases in response to the sight of the notes. Practice makes us independent of the notes because eventually the movement-produced stimuli of one phrase become the substitute stimuli for playing the next.

In the stereotyping of habit the first steps include movements that are merely chance responses to irrelevant stimuli of the first rehearsal. First attempts at reading include hand movements, foot movements, irrelevant lip and tongue movements. These tend to drop out with practice. The reason for their disappearance is that they were responses to irrelevant and adventitious stimuli which are only once or only occasionally present in the rehearsals. If, at his first lesson in reading, a small boy is holding a pencil, twisting his foot about the chair leg, grimacing, the second lesson may repeat these acts because they have been conditioned on the remainder of the situation. They will not always be eliminated. Only if successive lessons include stimuli to other conflicting irrelevancies will the first be lost. There is ample opportunity for this, however, because responses to irrelevant stimuli do not regularly occur in each lesson and one failure or one inhibition does away with them. The essential or relevant stimuli for reading always include the sight of the printed words. Responses to chance accompaniments disappear because they are displaced by responses to other irrelevancies. For the beginner learning the alphabet, "C" may stand for "cat" and insistently

suggest "cat." After a year has passed "C" stands for very little without its special contexts, because it has stood for so much in general. It cannot call up "cat" and "canary" at the same time because these are sounds made by incompatible movements. They cannot even be thought at the same time because thought, even if it does not always depend on movement and movement-produced stimuli, certainly is derived from movement. We cannot think what we have not once done.

Freiberg, Dallenbach and Thorndike (1929) have assembled evidence that the simple repetition of series of events in action or thought does not in and of itself lead to the omission of any terms of the series. The dropping out occurs, we may suggest, only when the terms are irrelevant in the sense that they depend on stimuli which are not essential to the action series, when they are not recurrent, and when they are replaced by stimuli to incompatible movement.

If, for example, we are engaged in any one of those repeated action series that lead to stereotyped habit, such as entering our home, the first occasion involves many movements dependent on adventitious stimuli. The postman is leaving and we stop to exchange a word with him; the dog greets us at the street and we respond to him. These are variable elements in the situation and responses to them are generally eliminated from our final habit series. These adventitious acts tend to be repeated, and are sometimes a part of the stereotyped habit, but they are eliminated if successive homecomings have offered other stimuli that break them up, or if some of the

external stimuli on which they depended are lacking. On the second home-coming the serial tendency to stop where we chatted with the postman gives way to our response to the sight of the entrance. We are not on this occasion looking at the postman. All that is left is the trace of the previous action which is a reminder of our meeting. We may think of the postman.

This reduction of habit to essentials makes of many habits local responses no longer involving the whole body. When we are practiced we can drive and talk, or play the piano and smoke, or skate and greet a friend at the same time. At first this is impossible because driving, playing, skating all include a mass of action that is not essential to the performance but is present because it is part of the total associated complex bound together by conditioning. In time, many irrelevant movements are dropped out from the complex and the activity is limited to the muscles and the movements required for the performance. This process is, of course, never complete. Perfect grace, which means the use of only the essential muscles and this use only to the point necessary for the action, is only approximated, never reached. A graceful skater relaxes the muscles not engaged in skating.

We are concerned here with the fixation of habit rather than with the development of skills, which will be the topic of a special chapter. Fixation of response, which constitutes habit, may stand in the way of skill as well as assist skill. But one feature of this fixation which contributes to skill is so characteristic of habit formation that it must be considered here.

Practice of a stereotyped action generally results in speeding up the action. This may take place even when it interferes with success. This speeding up of habit is not an invariable feature of habit formation, but only a very common one. Under what circumstances does repetition increase speed?

Habit series do not always speed up. Timing of a serial response depends on the movement-produced stimuli and the rate of movement depends on the number of fibers in contraction in the muscles used. The phase of the movement determines the pattern of stimuli from the movement. In action that has become fairly stereotyped speed does not, therefore, increase. But in action that still includes much that is "irrelevant" to the recurring stimulus pattern and hence much that is subject to adventitious change, the elimination of irrelevant movement may result in speeding up the serial response.

Besides this source of increased speed there is another. Many stimuli which have been acting for some time become conditioners of a later movement which is, as a result, elicited at the earlier appearance of the stimuli. Our behavior in general tends to anticipate the external event through conditioning of this sort. The chick first rejected the bitter caterpillar on tasting it. But while the movements of rejection were in progress the caterpillar was visible. On the next occasion the rejection takes place before the acceptance, and the result is avoidance. On the first occasions we may walk to the water-cooler, take a cup, draw the water, then drink. On later occasions the sight of the cup may start the raising of our hand before we have stepped close. The other hand is

ready for the tap before we have reached it. In the end the whole series has become one flowing action in which the originally separate acts are overlapped instead of distinct. Through such anticipation we learn to recover our balance before we are upset in a canoe. At first the upset is responsible for the attempt at recovery. The attempt at regaining balance takes place earlier and earlier until it occurs in time to prevent the catastrophe. It may even occur too soon and lead to overbalancing in the opposite direction. The beginner who has had a half hour's experience is apt to sit in the canoe with two opposed sets of muscles tense. The canoe is now in more stable equilibrium than the passenger, whose unstable balance is mental (by which we may mean due to learning). Fatigue may result in negative adaptation of this tension and leave the passenger with the slight and timely reactions that we call skill.

Dodge does not believe (1933) that this tendency to anticipate the event can be the result of simple conditioning, but I am convinced that from the simplest maintenance of equilibrium to the most complicated instance of foresight it can be reasonably maintained that conditioning is the fundamental process by which these results are achieved. In the last analysis foresight consists in reacting to signs of what is to come. The signs of rain are the substitute stimuli for a conditioned thought of an umbrella, that earlier depended on the shower but now is stirred by the clouds, the weather bulletin, or the rheumatic pain. Search for an umbrella may appear a very far cry from a conditioned salivary flow, but the maintaining stimulus that gives direction to that search

may be as simple a response as a subvocal murmuring of the word or a sustained readiness to grasp the object.

Speeding up a habit may be accomplished in a third way. When the habit is not adequate to its situation, when the machine operator is too slow for the machine, when the driver is not quick enough with his brake, the resulting confusion and its attendant conflict in response occasions excitement, general increase in muscular tonus, and so more energetic action. More energetic action means more rapid movement and, in so far as the activity is dependent on movement-produced cues, more rapid performance.

This speeding up through excitement may, however, introduce its own confusion if the speeding up is not uniform in the muscular system and may often cause the disintegration of a habit.

Unless irrelevant parts of a habit are eliminated (and it must be remembered that they are not always eliminated), unless overlapping stimuli set forward certain movements in the series, or unless interference produces excitement with a resultant quickening of movement, habits will not normally be accelerated by practice. In fact we may correctly speak of the *habitual speed* of an action since excitement, which is the only internal cause of acceleration among those described, is itself dependent on conditioning in that the conflicting action and interference responsible for excitement can be conditioned. The rate at which a movement is performed quickly becomes a habit unless it is altered in one of the ways just described (cf. Cathcart, 1928, 1929).

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Chapter XI

BREAKING HABITS

In the last chapter it was asserted that in repeated encounters with a disturbing event organisms tend to establish toward the event a stereotyped response that represents minimum exertion. This occurs through conditioning. Events that do not disturb serve only to facilitate the actions in progress and hence do not form new habits. Under what circumstances do such stereotyped habits disappear?

What has been said of inhibitory conditioning probably furnishes the answer. Established habits disappear only when new habits displace them.

The mark of an established habit is smooth action and highly predictable action. The habit is sure and well integrated. Each movement of it is a conditioner of other movements, which accounts for its comparative independence of external variation in the situation. As we grow older our styles and our mannerisms grow more in evidence and increasingly certain. The stereotyping may preserve waste movement and excess movement, but it tends to eliminate interference among movements and movements that result in disturbing stimuli. Where daily routine is fixed and dependable a stereotyped routine of

response insures minimum outlay of energy. Life is simplified. When we are young and in good health our ready recovery from fatigue and our physiological resources for excitement make this simplified routine unnecessary. Old age and ill health make mental health depend on the establishment of routine habits.

Pieron in *The Brain and Thought* described instances in which such simple, coordinated, well-established habits as making the sign of the cross were lost as habits although the movement could be performed without its former effortless grace. Brain injury was responsible for the loss. His conclusion that the habit had a specific "center" in the brain is not warranted because the wrecking of such a habit could be accomplished by the destruction of only a few of the brain pathways used in the cross-conditioning that makes up the habit. Some laboratory workers like Lashley have concluded from the fact that certain complicated sets of habits can survive the removal of any section of the brain, the complex system of habits being impaired in proportion to the amount of brain tissue removed, that brain pathways established through conditioning are not involved in such behavior. This conclusion is quite unnecessary because any habit is the habit of the whole animal, with all its sense organs and most of its muscles active. It is highly improbable that any one brain area is exclusively in use. Many parts of the nervous system are probably involved even in a simple action, and the destruction of a section of the brain interrupts some paths but not others. The

detailed movements of the habit will probably always be found changed by the operation.

Because the simplest habit includes conditioning of movement-produced stimuli as well as of exteroceptive stimuli to eyes and ears there are probably no brain centers that are exclusively used in any habits that include general activity. When only a few local groups of muscles are involved, as in speech, the situation may be different, and the loss of speech habits may depend on the destruction of limited areas in the brain.

This mistaken notion that a habit has its brain center has been responsible for many wrong opinions about habit formation and habit elimination. The simplest habit is a very complicated event and at first involves the whole organism. Only after much repetition is it localized in a limited number of muscles.

In this connection it is interesting to note two observations on which there is general agreement among psychologists. The first is that we are conscious of a feature of our situation when that feature is dominating the response of the whole organism. The second observation is that as a habit becomes stereotyped it tends to disappear from consciousness. We no longer notice it, and are unable to recall whether or not we have performed the act. It can now perform itself without attention.

How is it that these "automatic" habits can ever be broken? They are obviously hard to break. Under what circumstances can they be broken?

We have admitted that repetition is generally effective

in establishing a habit, but not that repetition is fundamental. An act is learned in the single occurrence. The need for repetition comes from the need for executing the act in a *variety* of circumstances. When most of the substitute cues are within the reactor's body, so that their occurrence as a total pattern is highly probable, habit may be established in one excited rehearsal. Tics are such habits, and their origin in emotional reinforcement has been pointed out by the Freudians. The effectiveness of repetition lies in the enlistment of new conditioners and in negative adaptation toward more and more potential distractions. Repetition is effective, but not necessary in breaking habits and in making habits.

The simplest rule for breaking a habit is to *find the cues that initiate the action and to practice another response to these cues.* No matter how well integrated a habit or how well it is established by practice, if the initial movements of the habit can be thus "side-tracked" the habit can be avoided. It will not cease to be part of the behavior repertoire of the individual because it will remain integrated. *Side-tracking* a habit in this fashion is to be distinguished from a thorough *breaking up* of the habit, which requires the unconditioning of the cues within the habit complex which serve to integrate and to stereotype the habit.

Avoiding or side-tracking a habit is easier than breaking up a habit. The chief difficulty in the way of avoiding a bad habit is that the responsible cues are often hard to find, and that they are in many bad habit systems extremely numerous. Each rehearsal is responsible for a

possible addition of one or more new cues which tend to set off the undesired action. Drinking or smoking after years of practice are action systems which can be started by thousands of reminders, and which become imperative because the absence of the object of the habit, the drink or the smoke, results in a block to action and so in restlessness and tension. The desire, which includes tension in the muscles used in drinking or smoking, disrupts other action. The writer who "wants a smoke" is disturbed in his writing and the disturbed state will continue until the aroused action tendency is allowed to go through. The original wakening of the desire may be caused by any of the chance accompaniments of previous smoking, the smell of smoke, the sight of another person smoking, or of a cigar, the act of sitting back in the office chair, sitting down to a desk, finishing a meal, leaving the theatre, and a thousand other stimulus patterns. Most smokers can, while busily engaged in activities not associated with smoking, go for long periods with no craving. Others find that the craving is strictly associated with such things as the end of a meal, if it has been their practice to smoke at that time. I had once a caller to whom I was explaining that the apple I had just finished was a splendid device for avoiding a smoke. The caller pointed out that I was at that moment smoking. The habit of lighting a cigarette was so attached to the finish of eating that smoking had started automatically.

When the cues for a bad habit are as varied and as numerous as they are in the case of smoking, it is clear that a general unconditioning of all cues is a long and

arduous process. A more successful method in dealing with such a habit is to "side-track" by attaching other responses to the initial movements of the habit itself. A good resolution is an attempt at this method, an attempt at the substitution of emphatic rejection with verbal reinforcement for the beginning of the act. Practice the beginning of the act with rejection instead of acceptance.

The smoker who has succeeded in avoiding or side-tracking the habit has not forgotten the art. The skill with which he can fill and smoke his pipe may survive many years of no smoking. The habit has not been forgotten, has not been broken up. This could only be accomplished by practicing the habit under circumstances that destroy its integration. This is more likely to happen to valued skills than to bad habits. The marksman whose practice has been semi-private may find that the beginning of a good score in a tournament, as soon as he notices it, proves a distraction and a source of stage-fright and confusion. Premature rehearsal of his triumph interferes with his aim. The confusion of his habits may outlast the tournament and set him back in his later practice.

The effect of punishment in the breaking up of habits is often miscalculated. Punishment is often not severe enough or not sufficiently distracting to break up the habit and results only in reinforcing the habit by making its performance exciting. Under excitement fixation is more rapid and certain. Much of the "naughty" behavior of children is thus encouraged by mild punishment and by moderate parental opposition. The interest in the ac-

tivity is heightened by its excitement, since interest is only more energetic pursuit of the activity. Drawing on the walls, saying "I won't," spilling food, uttering forbidden words, all take on the fascination that big-game hunting has for the adult and for exactly the same reason. There has been just enough opposition and excitement to fix the interest and compel the act.

Severe punishment is an obvious means of breaking up or of side-tracking a habit. Which of these takes place depends, of course, on the timing of the punishment. Punishment in the course of the habitual action may disrupt it. Punishment at the beginning may leave the habit intact but result in its deflection. Punishment after the act may have no effect on the habit whatever. What effect retribution does have depends on verbal cues which are associated with both act and punishment. In many cases the cues for the action and the beginning of the action do not call up the words or other attendants of the punishment, and the punishment remains quite ineffectual.

Since punishment gets its effect at the time through stimulating crying and struggling and thereby breaking up the unwanted habit, it is clear that any other means of breaking up the action pattern may be just as effective. Any interference that captures attention and introduces a new activity will be successful. Picking up a small child and tossing him or swinging him by the heels is just as effective in overcoming a balky fit as is a sound spanking, and, of course, has a great advantage in that it does not leave the parent a cue for frightened aversion.

The distraction must be so thorough that the muscle set of the obnoxious behavior which is the cause of its persistence is thoroughly changed.

Dunlap, in a recent book, *Habits, Their Making and Unmaking*, has proposed a very interesting and new technique for avoiding or deflecting bad habits, and reports the successful use of this method on a wide variety of actions, from errors in typing, facial tics and stammering to aberrant sex habits. His method he calls the method of *negative practice*. Contrary to all common sense he has the subject repeat over and over the undesired action. But this repetition must be supervised very skillfully in order to have the desired effect of doing away with the habit. Negative practice must be conducted in full awareness that the habit is to be done away with. In the case of a facial grimace or tic he requires that it be repeatedly produced by the subject and produced *before* it would normally occur. In the case of a persistent error in typing the error is practiced over and over, the subject being aware that it is an error. The result is that the typist can resume work and the error not appear.

These results have a very paradoxical look. They seem a direct contradiction of the older laws of frequency and of practice. We repeat an act in order to establish it as a habit. Are we also to repeat it in order to do away with it? Dunlap offers very little explanation for this paradoxical effect beyond calling it a new law of learning. Any explanation of learning, he believes, should be strictly avoided (Dunlap, 1932, page 314).

This is an astonishing statement for a psychologist to make and must be attributed to some very bizarre notion of what constitutes an explanation. A statement of the circumstances under which negative practice is effective and the circumstances under which it is not constitutes an explanation of negative practice. Dunlap suggests that the determining conditions are "thoughts, desires and ideals." Even if Dunlap has access to his patient's thoughts, desires, and ideals, in actual work with the patient he must use speech and gesture and action to accomplish his training. *Only the observable conditions under which learning occurs are of any use for a theory or for an understanding of learning*, and when these are described, the theory is already complete. One objection to Dunlap's own theory is that it uses unobservable conditions or conditions very vaguely described. We may, therefore, offer our own explanation, with the clear understanding that Dunlap, who is much more familiar with the application of his negative practice, would probably violently disagree with it. With Dunlap's opinion (1928) that the response has no effect on the future probability that the same stimulus pattern will produce the same response, or that the occurrence of a response actually decreases the probability of its repetition we must simply disagree. We may suggest that the phenomena of negative practice are illustrations of associative learning, rather than exceptions to the rule of association.

In the case of all these bad habits amenable to negative practice the reason they are beyond control is that the

subject is unaware of the cues which serve to start them. He can not tell what it is that begins the wrong series of movements resulting in writing "hte" instead of "the," or what the preliminaries of the uncontrollable facial grimace are. These unknown cues begin the action and, once begun, it is so well integrated through cross-conditioning that it is certain to go on. To use Aristotle's comparison, the response is like a ball which has been thrown and is no longer under the thrower's control. Dunlap's negative practice makes the cues obvious, or establishes other known cues for the very beginning of the activity, before it has attained momentum. To these cues can then be attached other behavior. The importance of starting the obnoxious action with full attention would indicate that the success of the method lies in re-conditioning the very first cues on which the habit depends. The subject can now "take it or leave it"; he has made it a voluntary act instead of an involuntary act, which only means that the course of the act will be subject to verbal deflection, or to deflection from the social situation. Our voluntary acts are acts that we can perform on request, or on our own request.

In dealing with children it is often more important to be able to ward off the formation of bad habits than to be able to correct them when formed. Nursery school and habit-clinic writers are now pointing out that a great deal that children do is aimed at getting attention, and that one of the most effective ways to keep the occasional misdemeanor from becoming a regular habit is to pay no attention to it. This is distinctly a purposive ex-

planation, an explanation in dramatic terms. It is a perfectly proper explanation as far as it goes, but we can go farther in stating the circumstances under which undesirable forms of attention-getting will be fixed as habits, and this more adequate explanation will be in terms of learning. A great deal of children's behavior from birth on is dependent on the attention of adults. Hunger, cold, pin-pricks, discomforts in general, continue to produce restlessness and crying which will last until an adult has given her attention. The infant cries until the adults about are disturbed and care for it. Fatigue may negatively adapt all the unsuccessful efforts and leave the discomfort a cue for the successful method of attention-getting.

Attention-getting behavior is thus acquired early and continues to be acquired in normal families. The child forms new needs for attention since its play habits may involve other persons. Play activities being initiated by the sight of others can not proceed until the other person gives his attention and takes his part. The result will be excitement and the ready fixation of habits which are effective in demanding attention. The bulk of the teasing, annoying, irritating behavior of children is fixed in this way, and random action that results in the startled attention of adults is very apt to be repeated. The complete disregard of a "naughty" performance on its first appearance is an effective way to avoid making it a habit. If the attention given it is in the form of scolding or mild remonstrance or argument, just enough emotional reinforcement may be furnished to fix a habit. A

"rise" out of the child's parents makes many an otherwise dull afternoon thrilling and interesting. His parents, themselves, if they are not aged, like a little excitement and risk or they could not have attained the status of parenthood.

The normal process of a psychoanalytic cure of a neurotic symptom has been described as practice of the cues for the paralysis, tic, hysteric "fit" or what not, with the general situation so controlled as to deflect the response. The psychoanalysts place emphasis on another phenomenon,—the tendency for the neurotic habit to disappear when the circumstances of its first conditioning are recovered in full consciousness. A recall of the forgotten occasion on which terror or syncope was associated with bright artificial lights frees the patient from the habit of responding thus whenever in a brightly lighted room. This is quite in accord with Nathanson's suggestion (1929) that bringing any automatic habit into consciousness removes it, and also quite reminiscent of Dunlap's description of the effects of negative practice, since one of the conditions for success in negative practice is a full awareness of the action. The old fable of the centipede that could no longer walk when asked the order in which its legs moved describes a very common human experience.

When attention is given to an act that has been performed without attention, the very act of attention is frequently a disruption of the habit. Attention to one of our own movements renders that movement different because it now lacks some of its cues. In writing we

ordinarily look at the words as they are formed, and there is no doubt that this determines in part the sequence of movements. If, in ordinary writing, we attend not to the written words as they are formed, but to the finger movements (which will be found difficult), the writing changes its character and becomes disorganized. There are conflicts and confusion. This is probably analogous to what happens in a psychoanalytic cure. The bad habit has depended on cues which were being responded to without attention. The recall of the distressing experience brings these cues to notice and so alters the response. This is not intended to mean an entire agreement with the psychoanalytic doctrine that an hysterical fear is always done away with by having been brought into consciousness. There are many cases in which embarrassment survives a full awareness of its origin.

Attention includes general movements of orientation, looking, listening, and so on, and includes also the inhibition of conflicting response. The stimuli attended to are by the movements of attention caused to dominate behavior and even a minor change in action resulting from this may undo the habit.

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Chapter XII

REWARD AND PUNISHMENT

ONE of the most ancient and one of the best established of beliefs concerning learning is the belief that learning is determined by its effects. Common sense puts this in terms of pleasure and pain. Action leading to pleasure tends to be fixed as habit, while action leading to pain tends to be "stamped out."

There can be no real quarrel with this popular theory. Children have been spanked or caressed on this theory for an undoubtedly long time. The theory is well established.

But the popular theory has several defects for scientific purposes. In the first place, when it is applied to animals we become a little puzzled as to how to tell pleasure and pain. And unless we can define them so that they can be unambiguously recognized they are of no use to a theory of learning; for a theory of learning must state the observable circumstances under which learning occurs and under which it fails to occur. Habits are formed by snails and earthworms, but no one has ever tried to describe what a pleased snail or a pained earthworm looks like. Pleasure and pain in dogs would seem a little more readily described.

We need then to substitute for pleasure and pain as the conditions of learning some less ambiguous condition, some condition that can be easily recognized by an observer, because we can not always ask the learner which he feels, and can not depend very much on his answer when he gives it.

This less vague and less ambiguous description of the conditions of learning Thorndike has suggested. He would substitute for pleasure and pain, satisfaction and annoyance. These are the conditions that determine the fixing and unfixing of habits. He defines satisfaction and annoyance in strictly behavioristic terms so that they can be readily recognized by any observer. "By a satisfying state of affairs," Thorndike says (1932, page 176), "is meant roughly one which the animal does nothing to avoid, often doing such things as attain and preserve it. By an annoying state of affairs is meant roughly one which the animal avoids or changes." His principle, which he calls the law of effect, is as follows (1932, page 176): "When a modifiable connection between a situation and a response is made and is accompanied or followed by a satisfying state of affairs, that connection's strength is increased. When made and accompanied or followed by an annoying state of affairs, its strength is decreased. The strengthening effect of satisfyingness (or the weakening effect of annoyingness) upon a bond varies with the closeness of the connection between it and the bond. This closeness or intimacy of association of the satisfying (or annoying) state of affairs with the bond in question may be the result of nearness in time or

of attentiveness to the situation, response, and satisfying event in question."

This statement of the law Thorndike has recently somewhat amended, in line with an admirable disposition to be guided by the facts rather than by his theory, a disposition which he has often demonstrated. He no longer believes that the action of annoyers is the opposite of the action of satisfiers in all respects. The strengthening effect of satisfaction is more universal, more inevitable, and more direct than the weakening effect of annoyance. An animal that gets food by pulling a loop learns to pull the loop. "But if an animal," he says (1932, page 276), "in the same situation pulls a loop and either (a) gets a shock in its paw at contact with the loop, or (b) gets a blow on the back, or (c) gets a sudden pain in the bowels, the weakening of the connections is likely to vary. In (a) there will probably be much weakening by way of strengthening the connection between the situation and the response of drawing back from the loop. In (b) there will probably be weakening, but less, because the reaction will probably be jumping away from the place, which is not so inconsistent with pulling at the loop. If the animal in (c) reacts by screaming without letting go of the loop, there may be no weakening at all."

In other words Thorndike's view now is that punishment may or may not lead to unlearning *depending on what it causes the animal to do*. The important thing to be noticed is that Thorndike's amended explanation is precisely in terms of the conditioning which he rejects

as an explanation. The sight of the loop will later cause the animal that hurriedly withdrew its paw on being shocked to withdraw its paw on the next occasion. The animal that gets a blow on the back will on the next occasion tend to jump away. The animal with the gripe in its bowels will tend to do whatever it did in response to the gripe. That this is an appeal to pure conditioning is concealed by putting the emphasis on the loop pulling and speaking of this as weakened or strengthened. We can be, as Thorndike acknowledges, much more precise than to predict the failure of loop-pulling. We can predict the recurrence of the specific behavior indulged in.

How it should escape Thorndike's notice that the effects of satisfaction are likewise readily and much more precisely described in terms of conditioning is hard to understand. Satisfaction is defined as a state of affairs the animal does nothing to avoid, or often acts to maintain. This is to say that a situation in which a maintained response is made tends on later occasions to evoke that response. "Doing nothing to avoid" a state of affairs does not mean doing nothing whatever; it means maintaining orientation, maintaining attention, and on the second occasion the general facts of conditioning would lead us to expect a repetition of the behavior.

In those cases in which the satisfaction is a satisfaction of some source of unrest, maintained hunger spasms or a continuous painful stimulation, it is quite reasonable to suppose that the precurrent behavior is associated with the hunger, but that this association is continuously destroyed by new associations. There is one act, however,

to which hunger may remain a faithful conditioner. That is the act of eating; and the faithfulness of hunger to this association derives from the fact that hunger dies when eating occurs. As Stevenson Smith and I pointed out in our *General Psychology*, elements of the consummatory response tend to be present throughout a series of actions driven by a maintaining stimulus. Hull's paper (1932) on his goal-gradient hypothesis described the rôle that such traces of the consummatory reaction may play in guiding learning and in holding the animal to its purpose.

Thorndike explains the appearance of retroactive effect, the reinforcement by a satisfying outcome of an association that lies in the past, by suggesting a physiological event in the brain. Of this possible event he says (1932, page 314) that "the evidence that it is some condition favoring conduction across certain synapses is still strong. The physiological equivalent of a connection does not thus vanish utterly in the twinkling of an eye. Whatever it is, it is there a second after it occurred in a manner or degree quite different from that of a connection of an hour ago."

This cerebral hangover is a highly speculative and quite unnecessary assumption. The physiological attendant of a connection which "does not vanish utterly in the twinkling of an eye" may well be the maintained muscular contractions of the response. We know that these tensions in systems of muscles may be maintained for long periods. And while they are still maintained they are reasonably assumed to be subject to conditioning.

The action that produces "satisfaction" is not over "in the twinkling of an eye," and by Thorndike's own definition of satisfaction it is an act that is maintained, or at least not broken up by the vigorous interference of punishment. Peterson in 1922 wrote: "Nerve impulses flash through the organism in but a fraction of a second. But there is considerable evidence to show that the effects do not so immediately fade away. Probably the responses of muscles and glands set up other nerve impulses, which . . . bring about further responses." And "these streams of impulses, therefore, will exist contemporaneously with subsequent stimuli and exert important directive influences on the nerve impulses these stimuli set up."

"The influence upon learning," Thorndike says (1932, page 312), "of both satisfiers and annoyers depends upon what they cause the animal to be or do." This is exactly what I am suggesting, namely, that the future response to a situation can be best predicted in terms of what an animal has done in that situation, in the past. Stimuli acting during a response tend on later occasions to evoke that response.

I would not hold that all satisfiers tend to fix the associative connection that has just preceded them. When a satisfying situation involves breaking up the action in progress it will destroy connections as readily as punishment. In teaching a dog to sit up, tossing his rewarding morsel to a distant part of the room will prove a very ineffective method. There is no doubt of the satisfying character of the meat. The dog certainly "does nothing

to avoid, often doing such things as attain and preserve," not, of course, the meat, but the eating of it. But the effect of the reward will be that the dog instead of sitting up stands ready for another dash across the room.

Just as satisfiers do not always "stamp in" a connection, so annoyers do not, as Thorndike himself has perceived, always "stamp out." What we can predict is that the influence of the stimuli acting at the time of either satisfaction or annoyance will be to re-establish whatever behavior was in evidence at the time.

"A satisfier," Thorndike says (1932, page 312) "which is attached to a modifiable connection always, or almost always, causes the animal to be or to do something which strengthens the connection to which the satisfier is attached; but we do not know what this something is. It may be to maintain relatively undisturbed the physiological basis of the connection; it may be to retain it longer than would be otherwise the case; it may be to confine it by some metabolic effect; it may be to alter it in some more mysterious way." I suggest that the mystery may be reduced by supposing conditioning to have taken place. This is, of course, to invoke another mystery but one somewhat less mysterious because more familiar. The something that the satisfier causes the animal to do on the second occasion is the repetition of its behavior on the first occasion—always allowing for possible new elements that may interfere. The dog's lesson in sitting up may be always interrupted by the cat. If it is objected that this explanation seems to demand a retroactive effect on connections we can only say that back-

ward association in the sense that the cue may follow the original stimulus is well established. This retroactive effect need not be actually anything more than simultaneous association. The substitute stimulus is probably always coincident with the response.

To this last quotation Thorndike adds a footnote: "The satisfying after-effect obviously often causes the animal then and there to continue or to repeat the connection." So long as the substitute cue for action remains, this would seem a very natural consequence. One taste leads to another because the stimuli are still present unless the animal turns away from the food, which would be by definition the work of an annoyer. As in the case of the chick and the cinnabar caterpillar, rejection, the result of the bitter taste, may be conditioned on the sight of the caterpillar, and replace the original impulse to peck. This is not a "retroactive effect" though it has that appearance. It is simultaneous conditioning. I venture to predict that learning would be much more uncertain if the caterpillar were of such a size that it was swallowed at one peck and not visible while rejection was going on.

We may go on to inquire how any stimulus becomes an annoyer in the first place, a question which Thorndike does not consider. He defines an annoyer as a state of affairs which the animal avoids or changes. But this ability to avoid is just what it is necessary to explain. Hammering the thumb or bumping one's head on a beam are not annoyers according to this definition unless we assume that the learning has already occurred, for the

victim can not avoid them after they have happened. If he avoids them at all it must be in time, and this implies that the learning which we hoped to explain has already taken place. We may, for the sake of argument, consent not to be annoyed by such events and deal only with annoyances which satisfy the definition, that is, with continuous stimuli which we can do something about while they are still upon us, such as intense heat, a bumpy road, thirst, hunger, flies, radio programs, or, if we are laboratory animals, charged grids, immersion in water, confinement.

Now we do not know, unless we observed it on some previous occasion, what either animal or man will do in any of the above situations. Holt (1931) has well argued that the early and primitive response to such stimuli is approach. If the fingers are flexed the finger tips touch the palm. The touch on the palm then comes to be a conditioner of the finger movement. Holt suggests that the grasping reflex is thus learned before birth. In general the stimuli caused by movements come to be the conditioners of the movements which cause them. Holt has also described how intense stimuli may, through conditioning, become stimuli for withdrawal and avoidance. Annoyers are essentially intense stimuli. Their original effect, before we have learned to avoid them, is in some cases approach, but in all cases excitement. Intense stimulation brings about general tonus of skeletal muscles and reinforces action. We do not know what a man or an animal suffering an intense stimulus will do, but we do

know that he will not be relaxed and that there will be variety and energy in his actions. He will be active, and being active his activity will be varied, because activity changes the stimuli which act upon him. Eyes and ears, as well as muscles and tendons will be subjected to rapid change in stimulation. If these intense stimuli responsible for his excitement are maintained (we have elsewhere referred to them as maintaining-stimuli) as they are in those situations which would satisfy Thorndike's definition, they have opportunity to become the conditioners of many and varied actions, but each successive action alienates them from its predecessor.

There is one act, however, to which these maintaining stimuli may remain faithful conditioners. This is the act which removes them. The maintaining stimuli are no longer present with the succeeding acts and so may remain conditioners of the movement which took them from the victim, or took the victim from them. If annoyance means avoidance, we had to learn to be annoyed at annoyers. At first they were only disturbers.

Dunlap treats reward and punishment (1932, page 30) in part in terms of feeling, rather than of satisfaction and annoyance. "That feeling in itself," he says, "without thought, is of any importance is improbable, except in so far as feeling may be an organic condition which is generally favorable or unfavorable to learning. From present information, we may infer that perhaps mild feeling is favorable and intense feeling possibly unfavorable, but we have no indication that any specific type of

feeling is any different in its effect from any other, except the feeling which is involved in desire."

To this it may be suggested that the effect of intense feeling is not unfavorable to learning in general but very favorable. But what is learned will be what is done—and what is done in intense feeling is usually something different from what was being done. Sitting on tacks does not discourage learning. It encourages one in learning to do something else than sit. It is not the feeling caused by punishment, but the specific action caused by punishment that determines what will be learned. To train a dog to jump through a hoop, the effectiveness of punishment depends on where it is applied, front or rear. It is what the punishment makes the dog *do* that counts or what it makes a man do, not what it makes him feel. The mistaken notion that it is the feeling that determines learning derives from the fact that often we do not care what is done as a result of punishment, just so that what is done breaks up or inhibits the unwanted habit.

My own view of the way in which unpleasant or unsatisfactory consequences of action affect learning might be further illustrated by a minor incident in the routine of a certain psychologist. He rented an apartment for the summer with a garage which had a large swinging door. From the top of the door hung a heavy chain. Opening the door hurriedly the first morning the chain swung about slowly and struck a blow on the side of the subject's head, a distinctly painful and "unsatisfactory" event. But this continued to happen each morning for

some two weeks. Why the long delay in learning to stand aside?

The answer, I believe, is that the act of opening the door was performed while looking at the exterior of the door. The chain struck after the door had opened and the scene changed. Dodging was not conditioned on the sight of the door because a sight of the door had not accompanied flinching from the blow. The flinching movement which occurred as the rear of the car came into view was too late. Only after the bruised ear became a chronic reminder and the incident had been talked about and finally had been told to a visitor on the way to the garage, did caution show itself in time.

The whole incident is not to be explained in terms of pain or annoyance, but in terms of the action and its cue. It is not the annoyance, but what the annoyed person does that determines what will be learned. Annoyance, in so far as it means increased muscle tonus and more complete and vigorous action, is favorable to learning to do whatever is done in response to whatever cues are present. The mistaken belief that annoyance discourages learning comes from placing all the attention on one line of action. Annoyance often accompanies the sudden disruption of an activity and leads to unlearning that activity, but at the expense of learning something else.

For an excellent account of the history of pleasure-pain theories of learning, the reader is referred to an article by Cason in the *Psychological Review* (1932). The theory had been elaborated by Herbert Spencer in

his *Principles of Psychology*. Cason's statement that to hold that what comes after an activity has a retroactive influence on the activity is a logical error, I can agree with only in a very strict sense. Through the facts that actions are spread out in time and that stimuli also may act for a period rather than for an instant, the actions that accompany pain or annoyance may be anticipated on a second occasion and so break up a habit series. Only in this sense is the pleasure-pain theory of learning correct. It is a very rough and inadequate description of the facts which are more adequately predicted in terms of the conditioning of specific behavior.

As the outcome of this discussion punishment and reward are not summarily to be ejected from the place they hold in public favor. No doubt whatever has been thrown on their general effectiveness. Children may still be spanked or caressed. But we shall have a much better insight into the uses of punishment and reward if we analyze their effects in terms of association and realize that punishment is effective only through its associations. Punishment achieves its effects not by taking away strength from the physiological basis of the connection (Thorndike, 1932, page 313), but by forcing the animal or the child to do something different and thus establishing inhibitory conditioning of unwanted habit. Punishment is effective *only in the presence of cues for the bad habit*. The law of effect would not have made us aware of this.

Furthermore, when the effect of punishment is only emotional excitement, punishment facilitates the stereo-

typing of the undesired habit. Punishment and reward are essentially moral terms and not psychological terms. They are defined not in terms of their effects on the recipient, but in terms of the purposes of the individual who administers them. Theory stated in their terms is bound to be ambiguous.

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Chapter XIII

SKILL

AN OCCASIONAL habit can be described as skillful, but habit and skill refer to two features of behavior that are very different indeed. In trying to define skill we must include a new dimension in the description of behavior. Habit can theoretically be described in terms of muscular contraction or of glandular secretion. To define skill we must make reference to something quite outside the organism and something not contained in the stimulus situation.

In other words the stimulus-response terms which we have been using up to this point now become inadequate for what we wish to describe. A habit is a habit, whether it is good or bad, successful or unsuccessful, but in one sense of the word good, no skill is a skill unless it is good. Progress in skill is progress toward goodness of a kind. Skill, in short, can be defined only in terms of success, of achievement, of a goal. *Skill consists in the ability to bring about some end-result with maximum certainty and minimum outlay of energy, or time, or of time and energy*. Skills are made up of habits, but habits stand in the way of skill as well as being the stuff of which skills are composed. Progress in skill is the formation of "good" habits and the elimination of "bad" habits.

The typical laboratory study of skill defines some achievement, such as hitting a target, getting cards sorted into their proper piles, typing a given number of words, reaching the end-point of a maze. The point of the experiment is the attainment of such an end-result. The movements by which it is attained are not recorded. So far in this book we have been concerned with the prediction of movement.

In the last few years probably more studies have been devoted to one form of skill, the learning of a maze, than to any other. The maze as used with rats consists of a system of runways, some of them "blind alleys," others part of the route to the maze terminus at which point is to be found (whether the psychologist speaks in terms of the Thorndikean law of effect or not) a reward, usually food. If food is to be the reward, the rat is usually hungry when placed in the maze. A record is kept of the time required to reach the food-box, and sometimes of the "errors" or wrong turns. But as a rule no notice is taken of the animal's movements,—only the result of the series of movements is noted.

Under such circumstances the rat on repeated trials normally reduces its time and the number of its "errors" and when these are represented graphically we have what is called a learning curve.

The rat must be hungry or it will spend its time (we are tempted to say, pleasantly) exploring the maze or scratching itself, or even briefly napping. Hunger is required to set the goal. Many psychologists conceive of the goal as a determiner of the learning. We can not do

this because we have resolved to remain scientists and to undertake to find the *antecedent* conditions that determine learning, and until it has been experienced the goal is not such an antecedent condition. When it has once been reached this previous experience does, it is very true, become one of the antecedent conditions. But this is not what psychologists like Wheeler mean when they speak of learning as goal-determined.

In our *General Psychology in Terms of Behavior* written some years ago, Stevenson Smith and I described this type of goal-behavior in the following words: "Many responses are of such a nature that they bring to an end the stimuli that caused them. Often, when a response is prevented, emotional reinforcement ensues, so that, when the stimulus is persistent or recurrent, negative adaptation toward it does not occur. This emotional reinforcement makes probable the occurrence of the response as soon as a change in the situation allows it. Such a stimulus may act throughout a long period, during which it interferes with responses to many other stimuli. Persistent or recurring stimuli whose responses are blocked with a resulting emotional reinforcement will be called *maintaining stimuli.* Maintaining stimuli are ultimately removed by the responses they themselves provoke. The final response that removes these maintaining stimuli, by altering either the external situation or the internal state of the animal, is called a *consummatory response.* The series of responses leading up to this final response are called *precurrent responses.*"

To this general view of the nature of drive or moti-

vation, which is substantially that proposed by Sherrington in 1911, I would still subscribe. A recent elaboration of it by Rexroad (1933) is so enlightening and clear that it deserves some review. Rexroad is considering the nature of goals and goal objects and the part they take in the determination of behavior. He suggests that goal objects may be divided into two types, "those *from* which the behavior is getting and those *to* which the behavior is getting." The first may be treated as simple stimuli. An animal on a charged grid, a bare-foot boy on a hot pavement, a man sitting on a tack have as their goals mere escape from the intense stimulation that causes general tension and restlessness as well as specific movement. These stimuli continue to act as what Stevenson Smith and I called maintaining stimuli until some movement carries the subject away from the source of stimulation, or the source of stimulation away from the subject. Goal objects *toward* which the behavior is getting depend on maintaining stimuli that do not come from the goal object. "Food," Rexroad points out, "is not effective for causing eating unless the animal is hungry, water is not effective for causing drinking unless the animal is thirsty . . ."

Goal objects toward which behavior is getting thus differ from simple stimuli in that the behavior toward them is "driven" by stimuli which are usually internal. The animal may as justly be described as fleeing from the acute spasms of hunger as well as seeking his goal, food. Rexroad continues, "A second difference arises from the fact that the direct response (the one made when the

goal-object is immediately present) removes the internal condition upon which the goal object is dependent for its effectiveness. Eating removes hunger, drinking removes thirst . . ." Goal behavior of both sorts is subject to ready conditioning because of the tension and excitement produced by the maintaining stimuli.

Goal behavior is the outstanding characteristic of living things. Haldane (1922) and Cannon (1932) have described life in terms of the maintenance of certain essential conditions of life. We can, after we have observed it, predict that body temperature, carbon-dioxide-oxygen balance in the blood stream and numerous other states will be maintained, though we may not know as yet how they are maintained. This holds true also of goal-behavior. We can as the result of experience with rats or men assert that certain goals tend to be reached under certain conditions. We can predict that a man will eat even when we can not predict how he will go about getting food. A number of psychologists like McDougall and Tolman and Humphrey hold that this striving for goals is the fundamental mode of human behavior and the behavior of animals. This is not to be denied. An organism is primarily a striving object. But even though we admit that goal-striving is fundamental in human and animal nature, it is the conditions under which goals are reached, and the conditions under which goals are established through learning that are the fundamental problem of the psychologist. If we attempt to understand goal-behavior we are at once driven to consider the nature of learning, the mechanics of learning, because in all

the more complex strivings of men and of animals it is through learning that goals are attained and through learning that desires, wishes, goals and purposes are formed.

There are many psychologists who hold that the nature of the pursuit of goals, aims, purposes is misrepresented by the preceding account. A goal is for such psychologists in some sense the *determiner of the action*, not just the outcome of the action. Now there is a sense in which goals determine action, but it is the contention of this book that such determination of action by a goal proceeds always through the previous association of the goal with action. The goal as a future event does not influence the action. It is evident that we must consider the nature of goals and their manner of determining action very carefully.

In the first place, how are we to know in advance what is the goal of any series of acts, animal or human? How are we to know, for instance, that the food at the end of the maze is the goal of the rat? Or that a performance in record time is the goal of the typist? I must confess at this point a certain awe of the psychologists who, like Tolman, say (1932, page 10) that "behavior . . . always seems to have the character of getting-to or getting-from a specific goal-object, or goal-situation." So much of my own behavior lacks this admirable quality, so often must I, when asked what I am about, seek desperately for a rationalization, that I am inclined to suspect even the higher animals of at least the occasional aimlessness which Wheeler, Tolman, and possibly

Humphrey would deny them. And when I do select a goal in advance it is so seldom that the outcome resembles this goal. Not that there is ever a failure to get to and get from places and things, since ubiety, or the property of position remains even in death, but that the things and places are so often not goals in the sense that they were selected in advance.

For a simple outcome of a series of acts to be a goal it is not enough that it be an outcome. Every event ends in something or other. Every action or series of actions has an outcome. For this outcome to be dignified by the name of goal it is necessary that it be planned by an agent. Men often do planning of this kind, and occasionally carry out their plans. But to attribute plans to rats, or to most men most of the time is to go beyond the evidence.

How can we know that food, or as Tolman has pointed out, a particular kind of food was the rat's goal when it was placed in the maze? What we do know in advance is, first, that the animal is a rat; second, we know that the rat has been without food for twenty-four hours; and this, coupled with the knowledge that this is a rat, will enable us to predict that the animal will keep in restless motion until fed or exhausted. We know by analogy that the rat's stomach is indulging in periodic spasms, and that the high points of activity tend to coincide with these spasms (cf. Richter, 1922).

We can with fair safety predict that the rat will reach the food eventually. We can also predict that on future repetitions of the performance the time required and the

number of blind alleys entered will be gradually and somewhat irregularly reduced.

We also know more than this, thanks to the work of Tolman and others. When Tolman says that at the height of hunger, the exploratoriness is specifically directed towards food, we must, however, be cautious. We know that food will stop the restlessness, but it is not true that activity will be directed towards food until food has been encountered on some occasion. If we have forgotten to place the food at the end of the maze on the present occasion this will have no effect on the rat's behavior.

Among the things we know from the experiments of Tolman and his associates are that the nature of the reward will affect the rate of improvement (Simmons, 1924); that a delay in giving the reward leads to less improvement than prompt reward (Hamilton, 1929); that a change in reward will disturb the rate of improvement (Elliott, 1928); that the degree of hunger affects the rate of improvement (Tolman and Honzik, 1930); that learning a maze by wading through in water will not materially affect the rat's ability to get through without errors when the rat has to swim (Macfarlane, 1930); that of routes differing in spatial or temporal length the shorter will eventually be taken (Gengerelle, 1925); that errors in the general direction of the food are less readily made than errors away from food (Tolman and Honzik, 1930).

All of this knowledge of the course of improvement of the performances of rats in a maze is the result of

observing rat behavior in a maze. It is derived from experiments of a new type, differing from those referred to in previous chapters in that these experiments do not record or attempt to predict the action of the animal but only the results of the action. The resulting generalizations are consequently of a new type also, and they do not, of course, contradict the generalizations of former chapters but they do represent extensions of our knowledge of behavior and of learning.

Tolman's theory in which he systematizes these results is, in a much abbreviated, and probably considerably mutilated form, about as follows: The white rat has, by virtue of his membership in his species, certain goal tendencies. These would include food, mating, escape from confinement, avoidance of certain types of noxious situations, nursing the young on the part of the female, and other such behavior outcomes. These are end-results which we can expect of the rat, knowing it is a rat. This is, of course, such a list as would have been called a few years ago a list of instincts.

Also we can be reasonably sure of more than this. We can be sure that these goal-tendencies will manifest what Tolman calls "docility." By this he means that repeated occurrences of a particular goal-behavior will result in an increase in the certainty of goal-reaching in the recurrent situation, and that there will be a reduction in the time required and in the energy required for the accomplishment of the result. In this one word, "docility," he lumps and dismisses all the phenomena which we have been considering under the name of condition-

ing or associative learning. The manner of this learning, he suggests, may be conditioning; however, it is not the manner, but the results of the learning in which he is interested.

So far Tolman's theory is the familiar instinct theory which has received a great deal of undeserved scorn in recent years. Animals do have their ways, and a description of these ways offers useful information. Tolman's theory does not stop with this. It goes on to make use of the history of the individual animal and to predict what the animal will do in terms of the situation in which it is placed and the experience it has had with that situation.

I have suggested that such information can only be utilized in some form of association theory. The form of association theory which Tolman has adopted does not use the association of stimulus with response, but the association of cues, which he calls "sign-gestalts," with a mental event. The early parts of the maze (Tolman, 1933) act not as cues for action, but as signs that the goal is to come, cues for the rat to expect the goal. By "expect" in this sense he does not mean action or preparation, but a mental awareness of some kind. His "sign-gestalt" formula leaves quite untouched the problem of what a rat will do with a sign, or of how signs are translated into action. "These sign-gestalt expectations," he says (1933, page 249), "I assumed would be to the effect that the earlier parts of the discrimination apparatus would have become a sign or a set of signs to the rats that the encountering of the food-compartments was to

be achieved by running through this discrimination apparatus." Signs, in Tolman's theory, occasion in the rat *realization*, or *cognition*, or *judgment*, or *hypotheses*, or *abstraction*, but *they do not occasion action.* In his concern with what goes on in the rat's mind, Tolman has neglected to predict what the rat will do. So far as the theory is concerned the rat is left buried in thought; if he gets to the food-box at the end that is his concern, not the concern of the theory.

This objection to the sign-gestalt theory does not at all apply to experimental work done by Tolman and his associates. That hungry rats do get to food boxes, that practice reduces the number of blind alleys entered and shortens the time required, that errors in the direction of food are more common than errors away from food, that certain problems are much more difficult of solution by the rat than others, are generalizations based on observation and make real additions to knowledge, particularly knowledge of the ways of a white rat in a maze.

Whether or not these laws are reducible to conditioning (which Tolman and the Gestalt psychologists would deny), has been the subject of a number of papers by Hull (1929, 1930, 1931, 1932). Hull defends the theory that the apparent "drawing power" of the goal is based on the conditioning of cues for successful movement. There is some evidence (Borovski, 1927) that errors are eliminated near the goal before they are eliminated at the beginning of the maze. The presence of hunger contractions with eating renders hunger contractions a possible cue for eating movements. Inhibitory conditioning does

not occur because eating removes the maintaining stimuli, which consequently do not become negatively adapted.

Such useless movements as entering a blind alley tend to be eliminated not because that is a general law of learning, but because blind alleys are left as well as entered, and always left after they are entered. The sight of them suggests leaving and not entering. Other irrelevant movements, like scratching, exploring, depend on chance stimuli which are not repeated at that point of the run on successive occasions and these adventitious movements are apt to be replaced by other movements on that account.

Not only do general movements of eating tend to be aroused by hunger contractions, but the specific movements demanded by the peculiar nature of the food are possibly in evidence. Hence when the rat runs the maze he is ready for whatever reward has been received in the past, sunflower seed or bran mash. This readiness is an actual muscular readiness, and is the actuality behind the "sign-gestalt" and the "means-end-readinesses" talked about by Tolman.

The "goal-tendency" or the purpose of a man or a rat is often made clear by his behavior. When hunger spasms drive the animal to action there is one action accompanied by the hunger spasms that is not succeeded by any other, and so is not subject to inhibitory conditioning. That is the act of eating. Eating removes hunger. Consequently hunger as a stimulus may remain faithful to those parts of eating behavior that are not

incompatible with walking, turning, and so on. The hungry animal may often be seen to lick its chops, swallow, and exhibit other features of the consummatory response.

When, in the course of a number of trials the reward is changed, confusion is introduced. Readiness for bran mash is now confused with readiness for sunflower seed and the two are as incompatible as readiness for a high and for a low pitch on the part of a baseball catcher. The rat, encountering the changed reward is disappointed, which is only to say that the movements with which the rat approaches the food are not movements adjusted to the eating of this particular food and there is some resultant confusion.

Thus Tolman has been very sagacious in pointing out that the rat does not learn the maze, but learns the way to sunflower seed or the way to water. And when the reward is changed confusion results. Furthermore when a rat has learned the maze route to bran mash when hungry and is placed in the maze well fed but thirsty he has a new problem. Thirst is not the cue on which the maze running has been based. He must learn the maze over again for water. His first practice with food as a reward is not a total loss because there are some action sequences whose serial form is ready to re-establish, but the primary maintaining stimulus that integrates the behavior and makes it *purposeful* is altered.

All of this discussion of rats and mazes may strike the reader who is primarily interested in human learning as a digression having very little to do with the understanding of the acquisition of skill. It is not a digression, be-

cause the central and necessary element in every skill is its purposefulness, its achievement, and rats, especially as a result of Tolman's work, have contributed to an understanding of the manner in which this purposefulness enters into behavior. Every purpose has as its essential feature some maintaining stimulus complex. In the rat this lay in its hunger spasms and in the muscular readiness for the familiar food. If this muscular readiness were broken up the rat might remain restless from hunger but it would "forget" what specific food it was after, and "forget" where to find that food. By "forgetting" we mean here only that it would fail to keep on the path to the food.

Maze learning in rats and men, and similar tasks, represent a very special and limited form of skill in which there is possible a highly stereotyped solution. Elliott (1934) has shown that under the drive of more urgent hunger the rat's path toward food tends to be more fixed and stereotyped. It is less open to distraction and inhibition from other habit tendencies. There is another class of skills which do not allow this solution in a single stereotyped series of movements. Skillful tennis, fencing, chess, games and sports in general, are very different affairs. In these skills there is no fixed order of action possible. No two games are alike in the order of the responses necessary. Skill here consists not in acquiring one serial habit but in acquiring thousands of habits fitted to differences in the situation. In tennis the player must learn a thousand different ways of returning the ball according to its speed, according to the direction

given it by the opponent. He learns to take his cue from the movements of the other player, and not to wait to see what the ball will be like. Remote conditionings must be acquired.

One difficulty in acquiring such skills is that the stimuli resulting from inadequate habits, the consequences of poor performance, do not take place in time to break up the bad habit. If it could be arranged that the stance and swing that result in "slicing" a drive in golf would in their course produce a sharp pain, slicing would quickly disappear. It does not disappear readily because the result of slicing is, in the first place, not immediate discomfort, which would break up the bad habit, but only a belated perception of a ball taking the wrong course. In so far as this belated perception leads to rehearsal with changes improvement is possible. If an instructor is present, his speech may accomplish the "break up" and serve instead of a pain.

Dunlap, in a discussion of improvement in skill (1932, page 34) in throwing a dart at a target, points out that in practice in which ninety per cent of the darts are thrown wild, "in the total learning, the darts thrown wrong probably have been as effective as an equal number of those thrown 'successfully,' provided the thought factor has been adequate." This is, of course, a very vague statement and the adequacy of a thought factor would be hard to observe or to control. Dunlap's account does not go far enough. Improvement resulting from wild throws depends first on what has been referred to by experimenters as "knowledge of results." But this knowl-

edge of results cannot be left as mere knowledge or it will be useless to theory. What it undoubtedly consists in is the perception that the throw was to the right, too high, or too low, not merely the knowledge that it was wild. And we may venture to believe that only in cases in which this perception of "too far to the right" includes a *change of stance* or a partial rehearsal of a correction will Dunlap's "thought factor" be adequate. This correction of posture or movement, this rehearsal with a difference breaks up the wrong habit and makes improvement possible. It is often noticeable on the golf course.

The golfer watching his putt keeps his posture. The "thought" that his putt was a failure will not bring improvement. Better performance will result only if the sight of the ball rolling too far to the right leads to rehearsal of a corrected swing. He may over-correct, but he has at least broken up the pattern of action that failed to reach his goal.

Before he has developed skill he addresses his ball tense with a fear of hitting too hard, and at the same time tense with a fear of hitting too gently. These "fears" are nothing more than conditioned corrections, resulting from previous errors. Only long practice will reduce the conflict of opposed muscle groups and deliver the ball with the minimum exertion that we call grace or skill.

This reduction of energy required is not a universal law of behavior, as Humphrey would have us believe, but it is fortunately a very common result of practice. And the part taken by the goal is not in the form of an

attraction or influence. The player has learned the object of the play. Set for a certain outcome or goal, failure breaks up the habit and brings a readjustment.

Learning the typewriter is learning to make certain movements at certain cues. As practice begins, the would-be typist pronounces the letters to himself and makes a somewhat tentative movement at the key. This movement is at first apt to be an extensive one. It may include a nod with each stroke. When the letter appearing on the paper is not the one muttered there is a repeated effort, with changed movement. This is a combination of previous habit and slight trial and error. The quick readjustment tends to break up the wrong habit and eventually the letters will be struck as muttered; movements that depended on adventitious stimuli will be eliminated because these chance stimuli do not occur twice the same, and there is a resultant speeding up.

With further practice combinations of letters often repeated are integrated into continuous movements and will be "set off" by the sight of the combination as a whole. Later, whole words that have been often written become in the typing of them single acts. The final development is for the seen copy to begin the adjustment for writing another word while the writing of its predecessor is still in progress. At each stage new errors are introduced and broken up. One of the commonest errors in the typing of a semi-skilled typist is to anticipate a word and begin it before the completion of the word or syllable preceding it. The wrong letter introduced into

the copy is in many cases one that should properly occur shortly after where it is inserted.

There have been many thousands of studies of the acquisition of special skills. For many years these were inspired with the hope of discovering the "curve of learning." When put in graphic form with the number of errors or rate of performance as the ordinate and with the number of the practice period or with the elapsed time as the abscissa, these curves encouraged the belief that there was some standard or ideal form of the curve of learning from which particular instances departed because of the special conditions governing the particular experiment. There was general agreement that improvement was more rapid at the beginning of practice; that as practice continued the rate of improvement diminished until a final limit was reached, a limit supposedly set by the physiological nature of the learner. This regular improvement was often interrupted by plateaus or periods of practice during which there was no improvement. Speculation concerning the nature of these plateaus suggested that they were periods when improvement was concealed in the form of increasing integration, or that they represented the physiological limit of performance with the methods in use and that the resumption of improvement was due to some radical change in method. Failure of incentive might also be responsible.

All of this discussion of the "curve of learning" parallels the investigation of the forgetting curve, which was also assumed to underlie the various forms of curves developed in experiments with different material.

There is, of course, no ideal or standard curve of learning or curve of forgetting. All depends on the nature of the activity learned or forgotten, on the amounts of previous practice of component activities, on the habits the individual may have previously formed and which may impede or facilitate a particular skill, on what is done during the intervals between practice which may attach other responses to cues used in the activity, and finally, since success is success however attained, progress is badly infected with chance.

There is, in other words, no general curve of learning. There are learning curves for special skills, like typing, telegraphing, skating, shooting, when these are practiced under standard conditions. A factory making jute bags may keep careful records of the daily output of beginners from the first day's work on. The number of bags sewn per day increases more rapidly at first. About the third week there is for most workers a plateau at which improvement is lacking or very slow. This is followed by a second rise. Under these fairly uniform conditions we do find that the learning curves tend to have a strong family resemblance. It is possible to estimate within reasonable limits what seventy-five per cent of beginners will be turning out at the end of six weeks. The piecework incentive is not uniform in its appeal to all but the variation is not extreme. It is possible also to assign the cause for the plateau, because the end of it in the cases where it does end coincides with the sudden acquisition of the ability to turn the corner of the bag to sew the second edge without stopping the machine.

Learning curves are thus specific and not general, and the interest in them has considerably lessened on that account.

Another discussion that has filled many pages concerns the question: Under what circumstances does practice in one activity affect favorably or unfavorably performance in other activities? This question was raised in several forms. Can, for instance, the ability to remember names be improved by practicing a specific list of names? Does exercise of the memory improve the memory in general? Or does it merely strengthen the items practiced? In strong revolt against the old conception that lay at the base of phrenology, at the notion that because memory is a name for a class of actions, memory must be a faculty and perhaps even have a local residence in the brain, many psychologists undertook to deny very vigorously that practice of one set of actions classed as memories would affect favorably or unfavorably another set of memories. Psychologists divided into two camps, with the skeptics in the majority. The debate extended far beyond the laboratory because it involved a theory of education, the theory that certain educational disciplines like Greek or logic might be lacking in direct usefulness but that they trained the mind in general, or certain faculties of the mind such as judgment, discrimination, reasoning power, or the like.

The dispute went on and in the meantime many laboratory studies were undertaken to answer the question: Can practice in one skill have effects that will transfer to other skills? The results of the experiments, while they

proved very disillusioning to the upholders of the theory of formal disciplines were not at all unambiguous.

Practice at skill A may affect favorably skill B, unfavorably skill C, and be of no effect on skill D.

At this point Thorndike proposed a solution. Practice in skill A affects favorably skill B because they have identical elements. Studying mathematics has a favorable effect on the ability to get good grades in philosophy because certain habits contribute to both abilities. A habit of refusing invitations to go to the theatre acquired while working at mathematics would favor good work in philosophy. The habits necessary for sitting still with a book for protracted sessions would be common to both ventures.

The general outcome of the discussion was that psychologists were willing to go so far as to advise students to practice as nearly as possible what they wished to be able to perform. Students are no longer advised to learn to think by studying mathematics and logic and then use the acquired thinking ability in other courses.

This remains fairly sound advice, but it does not settle the question of transfer. Thorndike's principle of identical elements did not do much to settle it either, since there is no method for counting identical elements or for judging their relative importance. We are forced to be contented with a position like that taken on the curve of learning. Special studies only will determine whether practice at mathematics affects favorably or unfavorably performance in philosophy, whether practice at golf improves or interferes with performance in baseball,

whether typing aids or hinders piano playing. There is no formula by which these questions can be answered in advance of experiment on the specific issue. There is no general answer to the problem of transfer of training. The writer is tempted to add that there is no general problem of transfer. The problem is always a specific one. There can be no doubt whatever that the person who has had a course of training in the classics, or in engineering is different from what he would have been without that training, but the nature of the difference can be told only by special investigation of the effects of the training on specific activities.

Several other generalizations like Thorndike's rule concerning transfer have been attempted. Experiment brought out the fact that in a number of forms of skill or of memorization the rate of improvement was markedly affected by the distribution of practice periods. Practice at typing for one hour a day developed a given number of words per minute with a smaller investment in time than did a practice period of two hours a day. Even with rats one trial a day in the maze produces more improvement per trial than do two trials a day. Memorizing a poem or a list of nonsense syllables requires less total time when one short period once or twice a day is used than when practice is condensed into longer sessions.

In general these results have stood the test of experiment on varied materials. The generalization is that of two arrangements of practice periods the one that uses

longer intervals up to perhaps two days has the advantage.

In the case of typing there is obviously a point in the length of a session at which fatigue introduces confusion and the breaking up of habits, so that beyond that point continued practice is worse than useless. What habits are being formed are apt to be bad and there may be a negative value in protracted sessions. Where this point is depends on motivation, of course. We suspect that added practice has negative value only from the point at which fatigue or loss of interest begins to introduce errors. Such errors may be established as habits and be hard to eradicate. There is no doubt that the student who continues study beyond the time when he is able to appreciate what he is reading becomes habituated to sitting scanning the lines of his book and day-dreaming. Listening to a boresome lecture or sermon makes it more difficult to listen to future lectures, whether good or bad. Having the radio turned on continuously makes it more difficult to listen to music. Goodenough and Brian (1929) have summarized the results of experiments on the length of practice period in the conclusion that ". . . the greatest efficiency may perhaps be gained by appropriate interruption of practice at times when the setting up of undesirable habits seems to be retarding practice."

Other than this there is no law of the distribution of practice. The effects of different periods will vary with the nature of the material and the emotional reinforcement of the activity. Ruch mentions (1928) among the

conditions affecting the results of massed and distributed practice in learning, the number and age of the subjects, the nature of the material, the length of the period chosen, the interval between periods, and the criteria by which the amount of learning is measured.

Much the same comment can be applied to the relative merits of "whole" and "part" learning. Is it more efficient to practice such material as a poem by reading it through from beginning to end or by memorizing it one stanza at a time? Is a piano composition most efficiently memorized when practiced as a whole or in parts?

The first results tended to favor "learning by wholes" and much was made of this in advice concerning many types of learning. This seemed the plausible result because when material is memorized part by part, after the parts have been memorized it is necessary to learn the order of the parts—the cues for each part. But here again conflicting results began to appear and the only generalization that can be made is that much depends on the material and the circumstances of practice. In learning piano pieces, for instance, R. W. Brown found (1928) that the "whole" method was most efficient except for the most difficult music. The evidence in general is that where the material is of such great length that fatigue may affect a single repetition, there is no advantage in the "whole" method. So again we must, before we can offer practical advice, find by experiment the better method for the material we have in mind. There is, of course, no doubt whatever of the wasteful-

ness of a very common practice in memorizing, namely, the rehearsal from the beginning to the end of a first section, then from the beginning of the first to the end of the second section, etc. This "overlearns" the first section and leaves the last in a precarious state.

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Chapter XIV

TRIAL AND ERROR IN LEARNING

My own views of how behavior through experience is adjusted to recurring events have been set forth at some length in the chapter on Skill. It would be quite unfair to the reader who has not read a considerable amount of modern psychological writing to give the impression that these views are universally held among psychologists.

The principal objections to analysis of learning in terms of conditioning have come from Köhler, Koffka, R. M. Ogden, and Wheeler, to mention a few of the more outspoken defenders of the point of view now called Gestalt psychology. An adequate account of the differences between the Gestalt psychologists and the views to which they object would require a book in itself. The chief issues can be condensed into much less space.

Unfortunately, much of the discussion concerns issues having little or nothing to do with science, which is limited to the concise description of the world as found and the use of its concise descriptions or laws for the prediction and hence the possible control of nature. The laws of science describe the *observable* conditions under which certain classes of events take place.

Another unfortunate aspect of the controversy is the frequent use of the device popularly known as the "straw man." A great deal of writing has been directed against views held by a mythical misguided person but not held by any actual psychologist.

We shall take, since he is the most ardent partisan, Wheeler as the representative of the Gestalt views. We may quote (1929, page 316): "It is evident that learning does not proceed by trial and error with accidental successes fixed by pleasure and errors eliminated by annoyance [by this Thorndike is probably indicated]. Likewise it is certain that learning does not commence with random movements which are subsequently eliminated by mechanical agencies [this would probably be Wheeler's characterization of the present account]. 'Trial and error' refers to an imposition of a goal upon the learner to which he is not responding." . . . "He (the learner) fails to repeat the unsuccessful movements because he has perceived a *different* goal; another remote end has been established. . . . The *successful movement is repeated for the same reason that it was made in the first place*, namely, when the stimulus pattern in which the goal figured presented relationships on the learner's level of insight, that is, fitted his level of maturation, since learning is a species of growth process, a given level of maturation is not only maintained, under optimum conditions, but continues to rise with respect to a given task until the limits set by the learner's organic structure are reached."

It should be remarked in the first place that no actual

writers use the phrase "random movements" in the sense in which Wheeler implies. All psychologists assume that any movements made by an animal are determined by the stimuli acting, the animal's history and its present state. Movements are random only when viewed in relation to some goal or outcome.

Nor is it clear what Wheeler means by "accidental successes." The word "accidental" does not refer to the event as such but to our ability to predict the event. The accidental is that which is not provided for in our rule. If these successes are not accidental, they must be indicated in advance by the circumstances. But a formula which would tell of all activities which are going to be successful and which are going to fail is unthinkable. This betrays a very serious weakness in the Gestalt psychologists' formula—success is determined by insight. Success occurs, Wheeler says, "when the stimulus pattern in which the goal figured presented relationships on the learner's level of insight, that is, fitted his level of maturation."

How either the observer or the learner can make use of this "principle" for discriminating good moves from bad ones is quite beyond the writer's own level of insight. Some of the learner's movements turn out to be good; some turn out to be bad. In advance of experience no possible general formula can tell which will be which. Only after the event can good moves be told from bad. But after the event is rather late for prediction. So far as the insight formula is concerned success remains accidental.

Let us examine one of Wheeler's applications. ". . . when you train your fox terrier," he says (1929, page 252), "to sit up and beg, you reward him with a tidbit . . . in order to provide the dog with a definite goal with respect to which it will execute the desired act. Dog fashion, it will then understand what you want of it for responding to food-goals falls well within the repertoire of dog insight! Dog insight is relatively feeble and slow of development, especially under artificial conditions. Consequently considerable time will elapse before it will make the movements just as you want them. Meanwhile it is making numerous other movements, all at about the same time, jumping, barking, and running around; which of these is to function as the 'tool' for the securing of the food is the difficult problem you have imposed on it. As the dog keeps trying, the insight into the problem grows. . . ."

This invites a great deal of comment. In the first place this running about and barking, etc., is precisely what writers on learning have meant by trial and error. And just why the success of sitting up when that does occur should not be called accidental from the dog's point of view is very obscure.

In the second place "growing insight" seems a very vague notion. Our whole investigation of learning is aimed at discovering *under what circumstances an act will be learned as a response to a given situation.* The only circumstances hinted at in this account are the exhibition of food and doghood of the dog. A growth of insight is promised, but this does not promise just what

the dog will learn to do, bite your leg and make you drop the food, look bored and so discourage lessons in silly tricks, or whatever else a bright dog might have in its repertoire.

In the third place, as a set of directions for teaching the act this is very imperfect. Consider similar advice given to a school teacher. To make pupils learn to spell a list of words the teacher is to establish a goal, such as escape from the school room, and then wait for insight to grow, which is promised if the pupils are at the proper level of insight. The most important feature of the proper directions for the dog's training has been omitted. The trainer should be advised to hold the food and possibly manipulate the dog in such a way that the dog will sit up. As he accomplishes this he should give his signal. The act can be rewarded by giving the dog the food.

The food is offered according to Wheeler "to provide the dog with a definite goal." There can be no objection to this provided we understand the nature of a goal and recognize that the sight of the food (because of past associations) gets immediate attention from a hungry dog, starts its saliva and movements of eating (because of past associations), inhibits responses to potential distracting stimuli about the room, and, because the withholding of food blocks the normal immediate eating, makes the dog alert and tense and so favors the establishment of habits.

Contrast Wheeler's prescription for training a dog to sit up with the directions in books on the training of

dogs. The practical trainer regularly emphasizes (1) the means for compelling the specific action desired, either force or persuasion, and (2) the necessity for giving the command as the act is executed. We may quote Whitford's *Training the Bird Dog*. "Force," he says, "is so applied that the desired thing to be done is the most natural thing for him to do, and once the habit of doing the desired thing is fairly well fixed, the force is no longer necessary" (page 30). "We use a check line in such a way, in teaching the dog to stop to order, that the order to stop, the check, and the enforced stop all happen at the same time" (page 30).

The "integration" of the dog's behavior on which the Gestalt psychologist places so much emphasis is quite evident. But this integration, instead of being an unanalyzable mystery, an irreducible first principle of behavior, is a matter of degree and is controlled by circumstances. The primary cause of the integration of the behavior of animals is the fact that they are "all of one piece." The animal cannot, like Stephen Leacock's knight, go dashing madly off in all directions because he is tied together by ligaments. There are other conditions of integration which have already been mentioned and in the case of the lesson in sitting up, the sight of the food effects a distinct new integration of the dog's behavior by capturing its attention and by arousing in the dog energetic tendencies (actual movement or muscle tonus) to eat, which dominate the dog's behavior.

Of explanation in terms of association Wheeler says (1929, page 266), "*But it is inconceivable that two sets*

of principles, based upon logically opposite and incompatible assumptions, should be applicable to his behavior. So much for a general comment on the laws of association." The opposite principles referred to are insight and association. It is reassuring to find him later on remarking that (1929, page 271) "recall is a response induced by certain of the stimuli that conditioned the original experience," or that "certain words function as stimuli for a great variety of others through proprioceptive channels. That is, thinking of words involves incipient movements of the speech muscles, and these movements furnish stimulation because the muscles are supplied with sense organs."

The truth is that the Gestalt psychologist must speak in terms of association or conditioning when he is explaining the occurrence of any *specific* behavior *because no other hypothesis has been so much as put forward.* The purposive behaviorist like Tolman and McDougall and the Gestalt psychologists like Köhler and Wheeler have suggested a theory of the circumstances under which certain end results tend to be accomplished without reference to the means by which they are accomplished. Association theory is a theory of the circumstances under which certain movements or certain responses will occur. The two sets of principles are, Wheeler notwithstanding, entirely compatible. They do not apply to the same fields of prediction. It is quite true that a hungry dog tends to find food *somehow.* It is also true that if we wish to know how a particular dog will go about finding food our best information comes from

his past behavior under similar circumstances. And this is to use an explanation in terms of association. Hull, in a recent article (1934) has an excellent description of the way in which an animal may acquire a varied repertoire of ways for reaching a goal when disturbed by such a stimulus as hunger. The path chosen on any particular occasion obviously depends on previous learning, and also on present obstacles.

Both sets of explanations are quite legitimate and give useful information. The insight level of a child is the subject of a great deal of research. The average ten-year-old can be depended on to solve problems of a higher degree of difficulty than the average six-year-old and the details of this are important. But a scientific account of learning is devoted to finding the circumstances under which new responses occur and new insights develop. And the Gestalt psychologists have given the problem of learning almost no attention. Wheeler in his Gestalt vein dismisses it with the remark that insight somehow grows or matures. Tolman is content with calling behavior docile or teachable and letting it go at that. Wheeler does not confine himself to his Gestalt viewpoint always, and has in his text a considerable account of learning, but this is conducted largely in terms of association as we have seen.

In my opinion, insight, if it were defined in terms of new successful response to a new situation, would be unpredictable. Science cannot deal with unique events. Only in recurring events can we discriminate between mere antecedents and necessary and sufficient anteced-

ents. In so far then as insight means the ability to meet a new and unique situation with an adequate response it must remain in the category of luck. The work of the inventor, of the creative artist will never be entirely accounted for by scientific explanation. We can as a result of observation of repeated cases say that in a given set of circumstances this action or that will follow (in a certain per cent of the instances) or even that success will somehow be attained in a given per cent of the cases, but we cannot say that in a certain unique situation we may expect anything in particular. If we could predict the thinker's conclusions we would have made the thinker superfluous.

There remain a few incidental features of the Gestalt doctrine which deserve attention. To his general statement of his theory of learning (page 271) Wheeler adds a footnote: "The theory supposes, with Köhler, that there is always a particular organization of stresses within the nervous system which in part conditions the 'recall' " (1929, pages 272 ff. and 311 ff.). A great deal has been made of this theory of the brain as a dynamic electrical field in which any change in detail alters the whole pattern. The theory is quite safe, of course, from experimental verification or disproof by any technique so far developed and as an attempt to state the circumstances under which action or learning occurs it is quite useless because it calls on unobservable determiners.

Another concrete example will make more clear the differences between Wheeler and myself. According to that author (1929, page 252) when a trainer whips a

bird dog that has crushed a pheasant "the punishment is applied to furnish a goal for the animal's behavior; it creates a special stimulus-pattern on the dog's level of insight. The goal is a situation to be avoided; carrying a bird gently comes to be the means by which the goal is avoided." In my opinion this gives very inadequate information to the trainer. What can be done to keep the dog from crushing the bird? One old device is to insert pins into the bird. Crushing the bird brings about stimuli which tend to open the dog's jaws. Success is a compromise between letting go and crushing. And the compromise becomes a habit. This can be accomplished without any awareness on the dog's part that carrying a bird gently is "the means by which the goal is avoided." The very fact that few of us can at all describe the way in which we perform skilled acts or have any notion in what our skill consists would show that insight is not the primary event. The greater part of human skills are developed without any awareness of the slight readjustments that correct our faults. We make responses to stimuli whose presence or absence we are unable to report at the time or later. The golfer will often volunteer the explanation of his improvement but it is only the credulous who take him literally. To assert as Wheeler does that all improvement is "at the level of conscious behavior" (1929, page 240) the writer believes rather absurd.

One further item. As an indictment of the theory that learning depends on conditioning, Köhler and Koffka both use a very familiar and well-acknowledged fact

that response, especially perceptual response and recognition, may be established to a visual pattern and then be found later to be elicitable by the same pattern in another size. The force of their argument is clear when it is realized that a child who has learned to name the letter H in his book can later on name it when it is written large on the blackboard. His acquaintances whose names are learned at one distance are recognized when they are farther away. And this means that as visual patterns on the retina they are stimulating *a different group of sense organs*, one not involved in the original experience. Here we have a case in which stimuli which *were not acting* at the time of a response appear to be acting as cues for that response. My view of this objection to the notion of the conditioned response was expressed several years ago in an article in the *Psychological Review* (1930) as follows:

"That we do respond to patterns as such is not open to question. And this would seem to involve the complete breakdown of any theory of conditioning such as is being presented, for at varying distances the actual receptors and afferent paths activated by a visual pattern must be quite distinct. The fact, indeed, cannot be questioned, though it should be noted that it is not a general or uniform occurrence. The child who has learned to read the raised letters on his blocks will not ordinarily recognize the letters when he sits on them. The effectiveness of patterns applies only within very limited fields.

"Is it not entirely possible that the method by which we come to recognize a face at different distances as that of one and the same person is essentially the same

method by which we come to recognize the rear aspect of this same person as his own back? In this case of recognition there is no question of similar patterns, for the back of his head resembles his face less than his face resembles the faces of others. If we maintain an attitude, or repeat a response to an object while that object is the occasion of shifting stimulation and of new stimulus patterns the maintained response may be conditioned on the new stimuli. Our response to a person at different distances is the same, with differences appropriate to the distance. Why may we not attribute this sameness and this difference to the sameness and the differences originally present in the stimuli furnished by our original behavior in his presence?

"If we accept conditioning as an explanation for responding appropriately to a person on hearing his footstep, which offers a stimulation pattern quite different from the visual pattern to which we previously responded, why should we consider it mysterious that the appropriate response could be called out by the stimulation of a quite different group of visual receptors? The fact that they have the same pattern is irrelevant.

"The Gestalt psychologists assert not only that we respond in similar ways to similar patterns, which we undoubtedly do, but also that we do this *without any opportunity for conditioning*, which the writer does not at all believe. In the case of the hen which performed its trick using the eye which had been blindfolded during learning it is entirely possible that the cues for the proper movement were not primarily visual, but were furnished

by movements connected with vision before the experiment was begun. Animals and man both have movements of skeletal muscles congenitally associated with vision. These movements may be in part identical for stimulation of either retina. If the act is conditioned on these movements, it might be elicited from either eye, without regard to which eye entered into practice."

Koffka points out (1924, page 100) another difficulty in the acceptance of conditioned-response explanations. This was pointed out by Stout also. A cat which has once freed itself by pulling a string with its foot, may upon another occasion use teeth for the same result. Here the cat has learned to do something but what it has learned to do is not to move its foot but pull the string by one means or another. The response does not employ the same muscles that were employed in practice and this can therefore hardly be called a conditioned response.

In the previous case stimuli assertedly *not* present with the response now elicit it. In the second case a response *not* associated previously with the stimuli is elicited.

This is not an isolated case, but is a common mode of behavior. We do learn to attain certain results as well as to make certain movements, and one method may be substituted for another. But this fact does not make it less probable that our action depends on conditioning. The essential of the situation may have been that the cat had learned to fix its attention (i.e. a muscular orientation pattern) on the string, and in the past the cat has taken objects looked at with claws or teeth on different occasions. The conditioned response was not the foot move-

ment (though the cat undoubtedly repeats its method as a rule) but the attention. This has been considered in the chapter on Reward and Punishment.

One further objection made by the Gestalt psychologists to associationism and we shall leave the subject. Köhler devotes much argument to the point that animals respond in general not to the stimuli, light, sound, touch, etc., which play such a part in the explanations of conditioning, but to relations between stimuli. Trained to choose the darker of two shades of grey, A, the lighter, and B, the darker, and then confronted with B and C, C being darker than B, C will be chosen.

This of course would not require experiment for its demonstration because none of our visual responses are to absolute values of the stimulus. As twilight comes on we recognize our friends although the absolute illumination of all parts of their faces is much lower. But Köhler believes that this is somehow fatal to the notion of association as dependent on stimuli—fatal to what he calls "machine theory." His implication is that the body considered as a mechanism could never respond to relations between stimuli but only to stimuli.

Now all stimuli are relations. This is the accepted use of the word. Physical forces in order to act as stimuli must be changing. To a constant force response very soon ceases. If we stare at a brightly colored spot with constant illumination we cease to see the color. A touch on the skin that is kept constant ceases to act as a stimulus. The stimulus is in reality a change in a physical force or a chemical state.

Moreover, even so simple a structure as a beam balance responds to the relation between the weights on the two pans, not to their absolute value. The right-hand pan will be depressed if it has a heavier weight than the left. That neural activity in any one tract is dependent on the relative difference between two stimuli, and that this difference should constitute what we have called a stimulus pattern is quite acceptable and does not seem to me to be any objection to the concept of association. All objects on the surface of the earth are continuously responding to the relation between gravitation and centrifugal force. We do not need, on this account, to credit them with any awe-inspiring ability to abstract or to integrate.

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Chapter XV

LEARNING WITH AND WITHOUT INTENTION

MANY recent studies on learning have been devised to bring out the difference between learning with intention and learning without intention. Nonsense syllable lists may be almost indefinitely repeated without being memorized if there is no intention to learn. A ritual may be repeated for years, but will not always be memorized unless the subject "made up his mind" to learn it. It would be absurd, of course, to claim that there is no unintentional learning. All of us have annoying habits that we had not intended to acquire. But the "intention to learn" undoubtedly affects seriously the ability to memorize and the acquisition of skill. Bryan and Harter (1899) found that telegraphers of long experience whose rate of sending and receiving had been stationary for years took a new spurt when an interest was shown in their rates of performance and reached new high levels.

If "intention" can thus affect the rate of learning or determine whether or not learning will take place it becomes necessary to inquire into the nature of intentions in general. We may be able to find what there is about an intention that makes it so important in learning.

Many psychologists with strong antipathies for the very earthy and glamorless language of behavior have asserted that human intentions can be known directly only to their possessors, that they cannot be stated in terms of action, that they are purely mental affairs. If this were all so, we could make no progress whatever toward the use of "intention" in understanding learning. Fortunately it is not so. The psychologist can define what he means by intention in such terms that he is independent of the subject's inner and strictly private knowledge of his intentions and can judge some intentions at least by external observation. The courts have been inclined toward objective interpretations of intentions always. Intentions testified to only by their possessors on the basis of strictly private information are not evidence in the courtroom. The court judges the intention of the accused from the record of his overt acts.

In our *General Psychology* in 1921 Stevenson Smith and I offered an objective definition of intention which made the essentials of an intention, (1) the presence of some maintaining stimulus complex, (2) the blocking of direct response which would remove the maintaining stimulus, (3) the presence of precurrent responses which "commit" us to action, and (4) the presence of a readiness for the consequences of the act, anticipatory behavior of preparedness. Our acts can be judged unintentional when we are not set for their consequences. We intended to do what we did if we are found prepared for it. Just as in court the presence on the accused of a gun and a mask is taken to show an intention to use the

gun unlawfully, or the presence of a railroad ticket and a packed bag shows an intention to make a journey when other behavior is appropriate.

It will be noticed that this account of the nature of an intention makes it out to be something very like what Tolman's rat, making its way through the maze, has in its preparedness for eating sun-flower seed. The dog at the kitchen door licking his chops is intent on eating.

What is there about the nature of intentions thus described that would affect learning? Such intentions serve the same guiding purpose served by the traces of eating in the dog or the rat. By them the behavior is integrated. Past action associated with these cues is called up. The general conduct is subordinated to the intention. Intent on eating, the home-going dog disregards those stray items of notice that would delay him if he were not hungry. He is, by his intention, kept to the point.

Can this be applied to human beings? If we are willing to go beyond what has yet been demonstrated in the laboratory, to what is on general grounds very plausible, yes. It is more than possible, it is highly probable that the difference between reading a prose passage with the intention of learning it and reading it without that intention is an actual difference in the nature of practice. One reader is practicing recitation as he reads; the other reader is not. One learner has his attention on what he is doing. The other has not. A lack of attention to an activity is a failure to get certain types of stimuli from that activity.

There is a phenomenon called automatic writing usu-

ally given some space in books on abnormal psychology, in which the subject, with writing materials in hand, is engaged in speech but at the same time encouraged to write. Occasionally the subject does write, while carrying on a conversation with the experimenter, or, in the cases where the phenomenon appears without encouragement from an experimenter, with attention not on the writing. Afterward, and this is the reason for calling the writing automatic, the subject has no memory of what was written. Some psychologists describe this in terms of a division of the personality, which is perhaps a legitimate description but not the only one applicable. We may prefer to believe that the movements of writing have led one movement to the next, through associations established in the past. But why is the writing not remembered, as we ordinarily remember what we write? Probably because it was not read, not followed with speech as it was written. We remember what we write in normal circumstances because we see it and read it as we write. The automatic writing, which we have not seen or pronounced, could be recalled only by the arm movements by which it was written.

The essence of an intention is a body of maintaining stimuli which may or may not include sources of unrest like thirst or hunger but always includes action tendencies conditioned during a past experience—a readiness to speak, a readiness to go, a readiness to read, and in each case a readiness not only for the act but also for the previously rehearsed consequences of the act. These readinesses are not complete acts but they consist in

tensions of the muscles that will take part in the complete act. Told that the electrodes you hold in your hand are about to be charged by current from an induction coil, the muscles which would jerk the hands away develop some tension. A sharp noise, or a tap on the shoulder will often set off the act of withdrawing the hands though there has been no shock. Often it would be possible to keep the hands in place only by tensing the opposed muscles and grasping the electrodes.

The defense of a man charged in a Washington court with shooting his neighbor was an admission that he set out to kill the neighbor. Armed with a rifle he took ambush behind some shrubs from which he could see the door of the neighbor's cabin. As he waited with his rifle pointed at the door he began to reflect on what he was doing and eventually decided, so he told the court, not to go through with it. The neighbor suddenly appeared at the door and the rifle was discharged. The killer had no memory of pulling the trigger. His attention was not on the trigger or the door until the sudden appearance of his neighbor released the prepared action. The court took a very skeptical view of this account but it was essentially consistent with what we know of voluntary action and intentions. We can not argue the legal aspects of the case, but psychologically the man had, and at the same time had not, the intention to kill his friend. His original intention had undoubtedly broken down except in one important particular, the readiness to pull the trigger.

The maintaining stimuli and the readiness of certain

responses to these stimuli do not constitute a full intention unless they are *dominating the animal's behavior*, or, what is the same thing, *are being given attention*.

The bearing that this discussion has on the rôle of intention in acquiring a skill may now be evident. A skill is a system of habits organized to further some specific end-result. It is defined by this result and is measured by the relative amount of energy or time taken to attain the result, or by the proportion of success to failure.

Practice at any skill then assumes an intention to accomplish something, to clear the bar at six feet in the high jump, to make the pencil drawing resemble the landscape, to plough the three acres by nightfall. Being built around an intention, every skill has a core of maintaining stimuli. These may change from one moment to the next. The tennis player must have a rapid succession of intentions dictated by the action of the opponent. Through this runs an underlying intention to win, to gain the cup, to make the gallery take notice. All these intentions betray themselves in anticipatory movements. The player's glance darts toward the point where the ball is to be placed. This eye movement not only betrays his intention but is usually necessary to the carrying out of the intention. It is the cue for the stroke. A great deal of practice would be required to look one way and deliver the ball elsewhere and even here there must be a fixed connection between glance and movement. This ability may be cultivated in order to deceive

an opponent and its cultivation means learning some other eye movement as a cue for the stroke.

Every person who has taken part in physical contests knows the important part taken by the intention to win. This intention is by no means a mental affair in the sense that it is not of the body—it lies in the way the game is played. Sportsmen refer to it as a psychological factor, and this is an entirely correct use of the word because it is a matter of learning, habit, and association. The intention to win means a set, a preparedness for taking points, a state in which the action indicated,—and by "indicated" we mean associated with the preliminary cues by past training,—is ready to go.

Skill at tennis means the accumulation of thousands of habit responses attached to the proper cues, the advance signs of the developing situation. It includes the sustained attention to the results which constitute the goals or objects of the game. Because attention is on these and because they are parts of the intention, or the events for which the player is set, any failure tends to disrupt habit and make improvement possible.

Continuous failure leads to the anticipation of failure, and what is sometimes called a defeatist psychology. The anticipation of failure follows regular failure because the action dictated by failure is eventually elicited by more and more remote cues in the manner described in the chapter on Time Factors in Conditioning, and the player may eventually enter the game prepared to lose.

Skills are formed rapidly in contests because a living and thinking opponent is capable of offering continuous

surprise. Struggle with an opponent is different from learning a maze or memorizing a poem on this one account. And surprise has as its main result the introduction of confusion and interference in action with a consequent building up of states of tense excitement. It is this lack of stereotyping that characterizes play and accounts for the excitement which gives play its drive.

So-called intellectual skills are not different from physical skills in the manner in which they are established. Playing chess and playing tennis are learned in fundamentally the same way. Both are contests and hence expose the player to the unexpected. When one player is so skilled that his opponent can no longer surprise him the game loses its interest. Both games require long practice because they offer a tremendous variety of situations to which responses must be prompt. Thinking is at a discount in both games because thinking indicates lack of familiarity with the situation—the absence of a prompt habitual reply to each move of the opponent. Thinking marks those situations which ambiguously condition incompatible movements and hence lead to a period of blocked action. During this blocking, tension increases and equilibrium becomes less stable. Luria's recent book, *The Nature of Human Conflict*, describes this state. Small children confronted with a choice, that is, with stimuli to incompatible movements, go immediately into confused action. The small hand touches one piece of cake which is then obscured, leaving another which entices and is touched. An adult, on the other hand, pauses, and the action is prepared and

organized before it is executed. The conflict has led to a state of mutual inhibition in which added cues may occur. Luria believes these to be primarily linguistic. These movement-produced cues reinforce one action or the other and the conflict is resolved. This is very like Thurstone's (1924) view that thinking is checked action and always develops out of overt action.

To return to the nature of chess, we may remark that the skilled chess player does very little thinking. Long experience has presented so many combinations on the chess board, and the confusion of failure has so successfully broken up bad associations and left the good that a glance at the board gives the cue for proper action. This explains why the skilled player can take on ten or twenty opponents at once and stroll from one table to another, give a quick glance, recognize the situation, and make a good move. The more thinking his opponents have to do the less is their chance of winning.

It may be incidentally remarked that chess and tennis are alike in another respect, but that tennis has here the disadvantage. Neither skill deteriorates seriously over a lapse of time in which there is no practice. Tennis loses more by such periods because some of the moves of tennis are parts of other sports or of the daily routine of an active man. Skill at chess will withstand long periods of no practice because the situations of chess are not approached in daily routine and so habits are not disrupted; cues are not alienated from their proper responses by being conditioned to other behavior.

Both games have a major purpose governing play—

defeat of an opponent. The opponent and his moves are the maintaining stimuli which dominate attention. Both games have minor and shifting purposes, the return of a drive, placing the ball in a hard spot, or taking an opponent's piece, maneuvering into a good position. These minor purposes are the player's transient intentions. Popular speech is strictly psychological when it describes the player as being intent on these minor goals. He is intent in that he has begun action associated with them. He is "tense" with them. In an important chess match this tension often results in muscular tremor. This tension of muscles appropriate to the action tends to maintain itself through circular reflexes until the action is inhibited by a change in the situation, and in addition to maintaining itself it tends to dominate behavior and integrate it.

Both tennis and chess are affected by what is called "confidence" or by its lack. Confidence is nothing more than preparedness for victory. Preparedness for defeat makes winning impossible because it means acting too soon as if defeated.

All football coaches and many boxing trainers take great care not to put their charges up against opponents too formidable. A series of football defeats or a few experiences with a rough opponent put the contestant on the defensive, which means only that he is now using caution before the occasion for it. The opponent's moves have become substitute stimuli for protective movements first called out by actual punishment.

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Chapter XVI

PERCEPTION AND THOUGHT

If we were to speak by the book we could not refer to hearing the street car, seeing that the day is cold, smelling flowers, feeling the house shake. The proper object of hearing is noise, of seeing, light, of smelling, odors, of feeling, not the shaking of the house but of ourselves. We see not the man, but the light reflected from one side of his body surface. We speak of hearing someone in the next room. What we hear in the strict sense is a noise which may or may not have been made by a person.

The simplest instance of perception is an elementary conditioned response. C. Lloyd Morgan (1930) uses the caterpillar and the chick as his stock illustration. The chick seeing the caterpillar pecks it and the bitter taste of the caterpillar stimulates the chick to vigorous ejection from its bill. But the light from the caterpillar is still stimulating the chick's eyes while the movements of rejection are going on and the sight of the caterpillar becomes a substitute stimulus for rejection. On the next occasion the chick has what Morgan calls a foretaste. It reacts as if it had tasted the caterpillar before it pecks, and this reaction inhibits pecking. The bitter taste has been anticipated. Vision now serves instead of taste to

produce the results of taste. This is perception in its most elementary form, a response to a situation which is in part determined by past learning, a response which includes certain features because these features were present on some former occasion together with some of the present stimuli.

This is to describe every perception as an instance of conditioning. We perceive the contents of the cup before us as coffee because this same stimulus pattern in the past has accompanied the taste of coffee, the odor of coffee, the warmth of coffee, and the mere sight of the cup now gives us a foretaste, a "fore-smell," a "fore-warmth" as the result of conditioning. The sight of the cup or the smell has become a substitute stimulus for former responses to the various stimuli that coffee may offer.

To this description of perception in terms of conditioning many very telling objections can be made. In the first place it is pointed out that the conditioned response is not just a replica of the previous response. The chick's movements of rejection at the sight of the caterpillar are not identical with the movements of rejection that occurred after pecking as the direct result of the actual taste. We quarrel with a friend and on the next meeting we do not reproduce our quarrel but avoid his glance or turn aside or speak with a polite restraint. We fall on the icy pavement and on later occasions, instead of falling, we walk cautiously when we see the smooth surface of the ice.

This objection to treating perception as a simple con-

ditioned response is a valid one. Perception is conditioning but not simple. The perception differs from the original experience but it remains true that in the perceptual response can be found movements which were present on the former occasion. Our careful progress on the icy walk includes tensions of the muscles used when we fell. Our hands and arms are ready to fly out to break the fall. The caution is a compromise between walking and the protective movements that would inhibit walking if they were carried out. The chick's avoidance of the caterpillar on sight includes some actual movements of active rejection, and it is the presence of these movements that has broken up the tendency to peck. Our coldness toward the friend with whom we quarreled is the result of the inhibition of our usual cordial greeting by quite diverse responses learned while we were quarreling. But it represents the resultant of conditioned quarreling and what is left of former cordial habits.

We may, therefore, still hold that perception is the result of conditioning, even though we grant that the perceptual behavior does not resemble any former behavior in its total appearance. The behavior of the present moment is affected by the total situation acting on the sense organs. As a totality it is unique because total situations are unique. Our only hope of predicting behavior lies in selecting in it elements that are not unique, that have occurred before.

To make our prediction, our data must be observable antecedents. They include past observations of the species, past observations of the individual, and present ob-

servations of the stimuli acting and the state of the animal. The behavior to be looked for from any stimulus is the behavior that occurred when this stimulus pattern was last presented. If we know nothing of the past of the individual before us we are limited to "instinct" in our prediction. We expect from him what other members of his species do in these circumstances. If what they do differs according to their individual histories, we cannot know which of these different responses he will make until we know his history. And if we know his history, our prediction, in so far as it is based on any one feature of the situation, will be that he will respond to this as he last responded. This is merely another way of stating the principle of conditioning.

A problem faces us immediately. What if different features of the situation would lead to different predicted behavior? To this there can be only one answer. Only if we have had experience with a similar conflict of these same features can we venture any prediction. If we have had such experience we may be able to say in advance that one feature of the response will be dominant or that the result will be mutual inhibition and excitement, or a compromise in which both responses have lost their distinctive features in combining.

Now of course every response is such a compromise, and it is only a question whether the altered nature of the conditioned response will leave it recognizable. There can be no general rule for this beyond the very vague one that the chances of a recognizable repetition depend on the number of conditioners present. There is

strong evidence that two cues are better than one, three better than two and so on.

Our prediction in terms of conditioning will therefore be somewhat halting and tentative. We can make no accurate count of the conditioners. Many of them, especially those stimulating proprioceptive sense organs, would be difficult to observe. Our predictions will have a large error save in cases like Pavlov's where the total situation is under maximum control. But with all this difficulty what small knowledge we can have about behavior in advance lies in our expecting a stimulus pattern to be reacted to as it was last reacted to.

A generation ago it was current practice to distinguish two sources of a perception, the present stimulus pattern, and a past experience. The contribution of the past experience was called apperception. It was sometimes supposed to be a contribution of the central nervous system to the total result. The experience had left "traces" in the brain. This distinction served very well for a very cursory description. Twenty persons hear the word "Italy." One pictures to himself an avenue bordered with poplars, a second maps of various sorts, another a portrait of Mussolini or the Coliseum, or a can of olive oil. The stimulus is much the same for all. The differences are "apperceptive" and originate in the various pasts of the hearers, vaguely believed to be somehow or other "stored" in the brain.

The more modern view is an improvement. The perceptual response, even in the form of a visual image, is a response to the present stimuli acting. But the present

stimuli include a great deal more than the sound of the word "Italy." They include as well the stimulus patterns of the responses to that word, or the responses prevailing when the word is uttered. The responses to the word depend on past conditioning (apperception based on past experience) but are occasioned by present stimuli. Perception is thus based on past learning and is occasioned by present stimuli.

The most important factor in perception is undoubtedly the influence *of what the organism is doing*, an influence exerted through proprioceptive sense organs in muscles, tendons and joints. This is the most important determiner of perception but it is at the same time very elusive from the observer's point of view and in many cases has to remain hypothetical. It is what Marcel Proust has used in explaining the sequence of memories as he lies in bed in the morning. A change of posture, a movement, a shift carries with it a changed memory setting and another detail of his past.

The most important service of the Gestalt psychologists has been rendered in the field of perception. They have made rather light of prediction in terms of association or conditioning, though they use it. They point out a general tendency in behavior that seems more or less independent of the history of the individual and his associative learning. This is the tendency for perception to be integrated, to "click" into place. We are, for instance, seated at a desk. Someone enters the room and utters something which we cannot "make out." Not until we notice the fountain pen in his hand do we suddenly real-

ize that he has asked us, "May I have some ink?" When our glance falls on the pen there is a sudden completion of the perception of his meaning. Up to that point we are puzzled. This has very aptly been called the "Aha! phenomenon," and is a characteristic of many perceptions.

We have the same experience at the telephone. Puzzled at the meaningless jumble of sound when we take up the receiver, we suddenly recognize the voice, and the meaningless jumble becomes an invitation to dinner.

Several years ago a student offered me a newspaper clipping which reported a conversation in a restaurant between a prospective diner and the waiter. It ran as follows:

F-U-N-E-X?
S-V-F-X.
N-E-M?
S-V-F-M.
L-F-M-N-X.

The reader is of course somewhat baffled. If, however, he reads this aloud, and happens to be familiar with the dialect which Milt Gross has so successfully captured in print, the conversation becomes suddenly meaningful. Our perception of it is conditioned on the sounds, but not on the sight of the letters. With most persons the minimal "forming" of the letters as they are seen is inadequate; it is necessary to speak them aloud.

Two students are standing at the foot of the stairway of a college building. One is heard to utter the sound:

"Squp." This does not "click" until we see that the students with one accord turn and ascend the stairs.

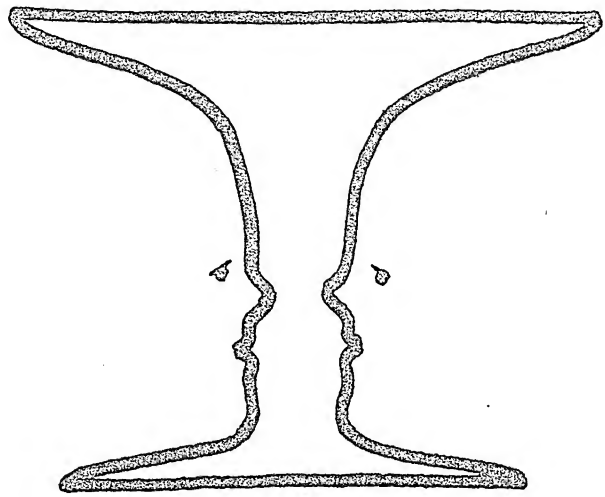
Most of the illustrations used by Koffka and Köhler are somewhat more simple than this. If a group of letters is exposed for a very brief time in an exposure apparatus, the subject tends to report that he has seen a word. Different subjects may report different words, but there is a marked tendency toward a perception of the fragment as a complete word. If a large segment of a circle is exposed the subjects tend to report seeing a complete circle. We overlook proof errors in printing very readily.

What accounts for this tendency to complete the perceptual response? In the writer's opinion it is essentially the phenomenon which Hollingworth has selected as the fundamental explanatory notion in learning—redintegration. A part of a former stimulus situation tends to evoke the complete response of the earlier occasion.

The reason that the response tends to be "redintegrated" (and this is the writer's difference with Hollingworth, who would explain conditioning in terms of redintegration and not *vice versa*) is that the elements of the former response are conditioners of each other, so that the response tends to be re-established as a whole if re-established at all.

The Gestalt psychologists have also pointed out that when a subject is confronted with a pattern, there is a distinct tendency for a part of the stimulus field to "stand out" from the rest as a figure, for which the remainder constitutes a background. This is for them a general law of perception, like the tendency to com-

pletion. For this general characteristic of perception an explanation in terms of conditioning and redintegration is also possible. Out of the patterns competing for attention one response or another tends to be redintegrated, that is, with an initial advantage once established, the



Ambiguous figure. One perceptual response or the other tends to be integrated. The figure is seen either as a wineglass or as two faces.

elements of the response reinforce each other and so competing responses are elbowed out.

In such ambiguous figures as the classical staircase, the outlined cube, or the wineglass we have stimulus patterns which have been associated with two different perceptual responses, a stair seen from above or from below, a cube with the near face below and left or above and right, a wineglass or two human profiles. *One response*

or the other tends to be integrated because any part of one response conditions the others and the union excludes the rival response.

These generalizations of the Gestalt psychologists and the experimental work on which they are based have extended materially our knowledge of perception. Their unwillingness to explore the effects of the past experi-

Lines (1) and (2) tend to be seen as enclosing a space. When lines (3) and (4) are added the space between (1) and (2) is seen as part of the general background and the figure becomes two enclosed spaces.

ence of the individual for the understanding of his present behavior is not so commendable. They are contented with pointing out the general tendency to integration of perception and with the demonstration of certain integrations which are almost universally made, and neglect the added understanding possible from learning. The rôle of past learning in accounting for common forms of perception has been set forth by Washburn (1926). Lines 1 and 2 are not mere lines, the Gestalt psychologist

points out. They appear to enclose a space. But the addition of 3 and 4 alters all this. Now the enclosed spaces are between 3 and 1, and 2 and 4, not between 1 and 2. Washburn (1926) would explain this through past learning. Our commonest experiences with these patterns are in boards, strips of paper, rulers, etc. In the right hand figure we are "set" to make two distinct movements to pick up the strips, in the left hand figure, one movement for one strip. The difference between lines 1 and 2 in the two figures is that the addition of 3 and 4 changes our response,—is associated with quite different action in our past.

We find ourselves thus in entire agreement with the Gestalt "laws" of perception, and would disagree only in that we would search for the origins of specific differences in the perceptions of different individuals in their individual histories of conditioning.

The "Click" or the "Aha" which the Gestalt psychologists have so much emphasized is subject to delay. We shall be not too far from common speech in using the term "judgment" for those cases in which the response is organized after a delay. The delay in a judgment, the failure of response to integrate and "click" occurs when the situation is ambiguous, when different features of it have been associated with conflicting and incompatible responses. Luria's account of this delay period has been mentioned. It is characteristic of the higher animals. Indecision is less common among barnyard fowls and mice than it is among men or apes. It appears during childhood and develops gradually. In

their capacity for it adults show extreme variation. It is not a trait that attaches to all of the behavior of any individual except under certain conditions. A man may prove a man of action in dealing with his business affairs but "sicklied o'er with the pale cast of thought" in his relations with his family. His decisions may be prompt when working with the tools of his trade but protracted and distressing when dining with more sophisticated friends.

Judgment is the outcome of conflicting perceptions, perceptual responses which are incompatible, which interfere each with the integration of the other.

What happens in the moment of delay that precedes the resolution of the choice? What behavior fills the interval? Luria states (1933) that children, like most animals, go into action immediately and do not exhibit this period of hesitation. Köhler has described the moment of quiet observation in his apes, followed by an adequate action. He calls this insight. What precedes insight? From the writings of Köhler and Wheeler we are given the impression that insight is just a "gift." You have it or you haven't it. There is very little to do about its absence but sit and wait for it to grow or mature, according to Wheeler.

It is quite obvious that insight depends on experience. Not only the immediate perceptions but the delayed judgment of an experienced physician are apt to be more adequate than those of a man without training. To some extent insight can be taught. It can certainly be encouraged by establishing opportunity for experience.

When we are confronted with a choice, action is blocked by mutual interference. What then happens? Probably something of this sort. Tension develops, so that any action that does go through is apt to be more vigorous. But during the interval of block both conflicting systems are in partial contraction. Each tends to call up associated behavior. This may facilitate one of the rival systems, inhibiting the other. Tension serves to make the balance unstable. The associated items are often verbal, and in their turn serve as cues for action which will facilitate or inhibit one or the other of the systems.

There is no clear line between thought and action. Whether the restriction of action through learning to minimal contraction of muscles can be carried to the point of neural activity without muscular contraction as Dunlap urges (1932, page 19) is still a matter of speculation. The possibility has certainly not been disproved. Studies on the minimal tongue movements accompanying the thinking of a sentence have shown the minimal speech movements present only in some of the subjects, not in all. I incline to the belief that some peripheral activity is necessary, some trace of muscular contraction which would furnish movement-produced stimuli for the continuance of the thought train.

My reasons for this belief are several. First, there is ample evidence of minimal action in a great deal of thinking. Perception, memory and imagination all are commonly accompanied by appropriate movements so slight they are not ordinarily noticed by the thinker.

Second, I am inclined to take seriously Jacobson's (1929) statement that his subjects found it impossible to "picture" objects without noticing tension in eye muscles, or to "think" a sentence without noticing tensions in speech muscles. Third, the manner in which one "idea" could bring up another "idea" is quite understandable if the first idea represents an actual stimulus pattern, which could act as a substitute stimulus for the following idea. How a train of thought could maintain itself otherwise is something of a mystery. Fourth, the speed of thought is not so great as it might be if thought sequences were dependent only on nerve conduction from one part of the brain to another. If the reader will think the series of numbers from one to fifty he will find that it requires an appreciable time, perhaps fifteen seconds. Why this delay? Even if no faint disturbances in the throat are noticed it is quite possible that they exist, and that the delay is explained by the fact that one number depends on the slight movement-stimuli in the throat as its cue, movement-stimuli furnished by thinking the number before. The fifth consideration is that action seems to be the precursor of thought. Verbal thinking in small children seems clearly to be prefaced by a stage in which speech is whispered or merely "mouthed." Being convinced that serial action of habitual behavior is dependent on movement-produced cues for its completion I incline to believe that thought series are maintained in the same way. A sixth argument lies in the ready convertibility of thought and action. What James wrote of "Ideomotor action" in which (*Principles*, Vol. II, page 522)

"movement follows *unhesitatingly and immediately* the notion of it in the mind" becomes much more intelligible if we find that the notion of action is a motor beginning of action. The question whether all thinking is to be identified with action remains open, however. The arguments cited are not compelling and involve some speculation.

If we accept this account of thinking, the distinctions between perception and memory and phantasy are made distinctions of degree. In all three the thought sequence is determined by substitute cues. In perception some of the important determining cues are what is seen or heard, are events external to the body. In memory and phantasy the thought is determined not by conspicuous external cues but chiefly by cues from the body itself. The memory of a scene is *behavior as if in the presence of the scene*,—not entirely "as if," since we do not usually dive into our memory picture of the lake or perform all the movements of eating when we recall the supper. We do, of course, often make a noticeable abortive beginning of the dive, or swallow frequently while recalling the supper.

Perception, memory and phantasy, all three depend on present cues. The memory is a perception of our revived behavior. The perception of a word before us is a memory of other occasions when the printed word became a cue for its response. The distinction between a memory and a phantasy is just that the memory is perceived as a revival, and the phantasy is not. All three are products

of learning, and if our view of learning is correct, of conditioning or association.

The account of thinking offered by the associationists had no adequate treatment of one of the central features of thinking—its purposiveness. For thinking is normally directed and to be understood in terms of a goal.

Thinking is, as the Freudians must be credited with reminding us, generally wishful, guided by desire, driven by a motive. What we see and hear is affected by our wishes; what we imagine, still more so. And our memories of the past are selectively evoked and generally distorted by our loves and fears and wants.

How shall we describe this wishful character of thinking and action? The psychoanalysts have undertaken the task by naming (but unfortunately not describing) sex as the general goal of all behavior, with minor goals or definite wish objects formed out of this general urge through associative learning as the result of experience. The Gestalt psychologists have described certain characteristics of perception, such as the tendencies toward completion and integration, and the general tendency to get whatever is wanted, or insight, together with a doctrine of tension and relief (Lewin), and a general tendency toward minimal exertion (Wheeler). Tolman would list certain goals which tend to be attained, together with a general "docility" in behavior which leads to the goal with minimum effort.

No one of these generalizations is wrong. Not all of them have the universal validity that their authors would attribute to them, it is true. Much behavior is aimless.

But the chief objection to most of these methods is that they are indefinite. They can neither be verified nor refuted by observation. Without additional explanation in terms of stimuli and associated response there is nothing to indicate what form the completion or closure will take, what path toward minimal action will be followed, why insight once integrated should be suggested on another occasion, whether the libido will express itself in highway robbery or a rivalry at pinochle. With association left out, the principal source of prediction of human and animal behavior is lost. The most certain and dependable information concerning what a man will do in any situation is information concerning what he did in that situation on its last occurrence.

As in action, so in thinking, goal seeking can be so described in terms of associative learning that our understanding of it is more complete and our anticipation of it more accurate.

Thinking often has a goal. What is the nature of goal-directed thinking? It would probably be much more correct to say that thinking generally has a topic. In so far as perception, memory, or phantasy are directed toward a goal this is because (as Rexroad has pointed out) stimuli are disturbing the organism. Perception is affected by wishes because the maintaining stimuli of the wish are part of the stimulus situation. The real goal is always relief from these maintaining stimuli. When we are hungry our ears are tuned for the dinner bell because the bell is associated with hunger; our phantasies deal with food; our memories concern meals we have enjoyed in the past.

Under the spur of hunger memories of indifferent meals become savory. They become savory because the slight disgust or nausea that is normally part of the memory is thoroughly inhibited by hunger. If instead of being hungry we are embarked on a rough sea, or have eaten too heartily, perception of the food about us changes, our memories and phantasies deal with former occasions on which we have been replete with food or disgusted, or nauseated. The lumberjack when he has finished his hearty meal often engages in an attempt to stop the appetite of the remaining diners with his stories.

Thinking has topics rather than goals. And the topic is set by the insistent maintaining stimuli through associations established in the past. Appetites and desires are specific only through learning. In the infant without experience hunger is a general restlessness indistinguishable for an observer from the excitement and restlessness that follow a pin prick, or falling, or being firmly held still, or having the edge of a card drawn along the sole of the foot a number of times. Hunger in an adult is directed into the lines of past action when hungry. It is not a vague restlessness but a specific interest in a particular dish at a favorite restaurant, or if not that, a conflict between tendencies to set out for two or more places where hunger has been allayed.

It is true that, once such a specific desire is aroused, there may be great variety in the means by which it is reached, but all these means, in so far as they do not depend on trial and error, which means on associated behavior modified by the situations encountered, are ini-

tiated through association, that is, as conditioned responses to the cues from the stomach. And thinking as well as action depends on conditioning in the same manner. Thinking occurs where action is blocked, but it is none the less a conditioned response to the acting situation, including the situation within as well as the situation without. Thought is as much ruled by habit as is behavior.

A specific goal, a desire, is a line of action started which cannot be completed. It persists as long as the fundamental disturbance persists, or if it lacks a persistent maintaining stimulus like hunger spasms, it may, once started, maintain itself as a posture is maintained. Desires of this latter sort can be erased by conflicting action, forgotten because broken up by other behavior. If it is a mere appetite aroused by the day's routine, we may forget our desire to eat in a book, or in a conversation. We are sure to lose it if the house is discovered to be on fire or we fall down stairs. *Nor is this loss any change that has occurred in the goal. It is a change in stimulation.* We have lost the desire and the goal has ceased to be a goal because the maintaining stimuli have been withdrawn. The psychologist who had placed his trust in the goal as the indicator of action to come is left in a very embarrassing position.

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Chapter XVII

CONCLUSION

PIERRE JANET, in his fascinating series of lectures published with the title, "*L'Évolution de la Mémoire et de la Notion du Temps*," distinguished memory from the *restitutio ad integrum* or redintegration which we have been describing in terms of conditioning. Animals, he says, have motor habits set up by their past experience and evoked by reminders of that past experience, but they do not have memory. The distinction lies in the fact that a memory is not a repetition of a past action but is an adaptation of the reconstituted behavior to a present social situation. Its roots lie in narration. A small child may show fear at the sight of a dog that has on some former occasion barked at the child. This is not, in itself, a memory. The beginnings of memory in the child lie in the beginnings of its narration of the event. It is from the time that the child turns from the event and says "I'll tell mother" that true remembering dates. We remember those events to which we give a special kind of attention, those events which we observe in order to narrate. The contention of some of the psychoanalysts that the very early experiences of infancy are forgotten because they are so tinged with lechery that later social

training drives them to the refuge of an "unconscious," Janet heartily rejects.

This is very much in accord with Watson's statement that the "unconscious" of the Freudians is really the "unverbalized." Our memories are organized about verbal cues. Our access to past events is normally through their verbal associations, and if these verbal associations were not made at the time of the event they will not serve to "call up" the memory. We shall not be able to tell what happened, nor shall we be able to remember it in the strict sense of that term. We may re-enact it, but the re-enactment is not remembering. Remembering is a highly complex social act which involves language.

Because it is a highly complex act Janet is led to assert that it is not, like motor habit, merely an affair of association. This contention is quite right; but it is equally true that no behavior is merely an affair of association or conditioning. We have said before that the principle of association or conditioning *is not an explanation of any instance of behavior*. It is merely a tool by which explanation is furthered. A tool is not true or false; it is useful or useless. There is no moral compulsion to use any specific tool; that lies in the discretion of the workman. The contention of this book is that we may profitably search for the signs or forerunners of an act among the stimuli which have been present with the act. The complete explanation of any act is an achievement which is helped on by such a principle, but it is not the principle that makes the explanation, any more than it is the hammer that builds the house. In the hands of a psy-

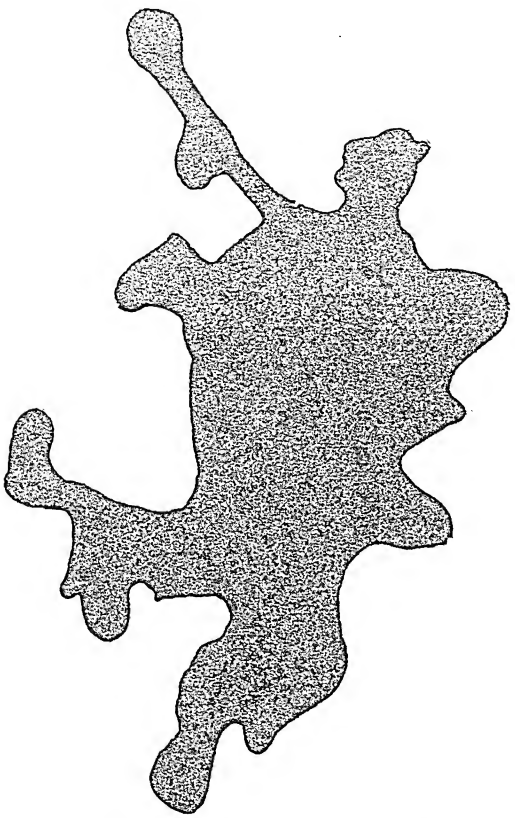
chologist willing to use it, the notion of conditioning is a profitable instrument; no one can compel a psychologist to use the notion against his will.

The problem of memory is not, therefore, "explained" by the notion of conditioning. We shall understand memory only when we have undertaken to observe its beginnings in children, or its aberrations in adults and then its normal operation. This is also true of the nature of perception, to which the Gestalt psychologists have given most of their attention. This book can make no claim to have solved the problems of memory or of perception. It is merely an argument that in attacking those problems we should not lose sight of the fact that past associations of response and stimuli are of use in predicting behavior. Conditioning is not the only ground for the prediction of behavior. Human nature is another source of prediction and control. Those reactions to changes in the environment which observation shows to be made by persons without reference to their individual histories we can predict without reference to individual history. In many cases, however, such prediction from membership in the species can be supplemented and made specific by adding associative learning. McDougall, for instance, states that human beings are naturally imitative. Now it is true that we can observe in all cultures many instances of the repetition of seen acts. Persons have been observed to laugh when others laugh, or to cry when others cry, or to whistle a tune on hearing the tune. But it is equally true that human beings are as a rule not imitators. We do not imitate the barber; we

submit to his shears. We do not whistle all the tunes we hear, but only those that we have previously learned. New popular tunes are new only in name. If we inquire the circumstances under which imitation does and does not take place we shall find the answer in terms of previous associative learning.

McDougall's principle that human beings are naturally imitative can then be refined in terms of conditioning. If we have performed an act, the stimuli associated with that act tend to become cues for its performance. We laugh when others laugh. Not always, of course. We may have fallen down stairs, or forgotten our cravat, or backed off the lecture platform and so stirred others to laughter. But without strong deterrents like these most of us can be moved to laughter merely by hearing the laughter of others and without "seeing the joke." The reason we laugh on hearing others laugh is that the sound of laughter has always accompanied laughter, and thereby becomes a cue for laughing. If we have been often laughed at when discomfited, such laughter brings only annoyance.

The Gestalt psychologists have pointed out that a perception tends to be an integrated whole. A glimpse of an incomplete circle or triangle will be seen as a circle or as a triangle. We tend to read meanings into what we hear and what we see. The clouds, or the figured pattern of the wall paper display patterns of familiar objects. In the ink-blot shown we may see a human head facing the right and with mouth open, or a dancing figure with



Ink-blot figure which may be seen as a face with open mouth, or as a dancing figure with arm extended. What is seen depends on the previously acquired repertoire of perceptions of the observer.

a gross body and tiny head facing the left. In such a figure a skilled artist will discover a dozen possibilities to everyone discovered by the man who has done no drawing.

This generalization of the Gestalt psychologists is true and interesting. But its significance is multiplied many times when we discover that what we make of the ink-blot, or what we make of the incompleted figure or of the garbled letters depends on our repertoire of previous learning. The ink-blot suggests more to the artist than to the layman because as the artist explores it, it has more associations with incomplete drawings of his own.

Psychologists who, like myself, speak the jargon of conditioning and association have been recently reproached for describing behavior in terms which leave out the main actor in the drama, the person who is giving the performance. We are charged with representing conduct as the result of stimuli alone, with no recognition of the person who responds to the stimuli. A man, for example, attends a summer lecture and in the course of it yawns. On the incoming air current rides a gnat which strikes a sensitive receptor in the throat. Nerve impulses are set up which travel to the central nervous system and out over motor pathways to diaphragm and intercostal muscles and result in a violent cough. The gnat is expelled. But he seems to have expelled himself, much as one might touch a button and turn on an electric fan. Mr. John Doe, whose throat it is, seems not to be involved at all. The incident appears only as an unfortunate escapade of the gnat's.

This is a very mistaken impression of the views presented in this book. The fact that a man is a living organism has, perhaps, been too much taken for granted; but it has been taken for granted, not denied. As a living organism, man shares the characteristics by which living organisms are defined, the tendency to maintain his pattern of structure and action for that brief interval which we call his span of life. In the higher animals, as distinguished from certain plants and from those organisms which never properly die, but only divide and live on, death and the breaking up of patterns of structure and action is as essential a characteristic as is the brief maintenance of integrity which constitutes living. As a living organism man is also a member of a species, and species of plants and animals as well as individuals resist destruction and adapt themselves to adverse conditions.

The use of adverse conditions to bring about the removal of the threat of disintegration, Stevenson Smith (1914) has called *regulation*. In our *General Psychology* we pointed out that responses are in general regulatory in this sense. They meet successfully the situations which contribute the stimuli that call responses forth. Sneezing is occasioned by a foreign object in the nose, and sneezing removes the object. A baby cries when it has been without food for a long period and its stomach is consequently active; its cries generally stir an adult to come to its aid. We are marvellously made and an astonishing list of vicissitudes is provided for in advance by our inherited structure.

The mere enumeration of the known regulatory mechanisms which achieve our protection from danger and maintain our integrity for our term of years would be a long task. The strategic placing of our sense organs for the reception of important classes of stimuli, the structure of sense organs which limits and further selects the stimuli to which they respond, the wonderfully adapted system of connections between sense organs and organs of response, the physiological structures which maintain our living cells in a fluid medium of astonishing constancy in temperature and chemical balance, the structure and placing of muscles and glands through which we respond to changes in the world and in ourselves, all these regulatory mechanisms together make up a total which will never be adequately described. But the most remarkable provision of all is the intricate nervous system which not only makes possible adaptive direct response to noxious stimuli and to the disturbance of those conditions essential to life, but also enables the organism to widen the field of stimuli to which it reacts *and to make its adaptive changes in response to the mere signs of harm or to the mere signs of relief*. Through associative learning responses may be elicited by new patterns of stimulation; new combinations of response may be integrated into stereotyped acts; the organism may even cease to respond to stimuli with responses which fail to relieve it of strain and substitute new action more adequate to preserve its integrity.

All this is characteristic of living organisms which we speak of as having "minds." It is so characteristic of

them that the description of *what organisms can do* appeals to some psychologists as the sole task of biological science. They will be contented with a catalogue of the results which an almost hopelessly intricate structure will bring about. In the realm of behavior this amounts to a list of instincts or instinctive capacities, and the compilation of such a list is of undeniable importance. Chief among these accomplishments is self-preservation. Most of the other capacities or abilities of the organism are instrumental to this end, except those that serve another end which is ultimately incompatible with this, the end of the preservation of the species which can be achieved only by the death of the individuals. Continuance of the species might be listed as the supreme end and self-preservation its chief minor goal. Contributory to self-preservation are such tendencies, capacities, instincts, or whatever we wish to call them as food-getting, defense from a wide range of dangers, the maintenance of water-balance, the maintenance of the temperature necessary to life. Learning is, according to Humphrey, a process of making a systemic adjustment (1933, page 102). Self-preservation is the fundamental law of learning.

It is quite true that the whole significance of all the biological sciences is lost if we overlook the fact that living things are organized to accomplish this end. But it is equally true that we shall have very little biological science and very little control over learning if we are content merely to point this out. The same end is served by various other means. Our lives are protected by many

devices other than learning. Invading bacteria provoke physiological changes in the blood which lead to their destruction. Sometimes this occurs in time. Sometimes it does not occur at all. If we have not been contented with a broad statement of the *vis naturae curatrix* but have discovered something of *how the result is accomplished* we may be able to interfere and assist the immunizing process. Dust breathed into the lungs is passed upward by the cilia of the walls of the lung passages and eventually disposed of by a cough. But not always. Haldane discovered that in certain occupations the dust was not thus eliminated and that one essential of the process was that the dust be of a certain coarseness if the cilia were to be activated. A cure for the effects of fine dust was to breathe in coarse dust which would start the ciliary action.

We must know *how learning occurs* as well as know *what it accomplishes*. Learning cannot be identified with what is fortunately its common result, systemic adjustment, because learning may be responsible for maladjustment and early death. Explaining learning in terms of its hoped for effects is to leave us helpless in its control. And to make the hoped for effect the fundamental law of learning, or to exalt it into a universal "law of nature" is to deny awkwardness, mistakes, failure, and death by the simple device of not including them in the universal law. There have been religious cults which maintained the reality only of the good, but there have not before this been scientific cults which have attempted this solution of the problem of evil.

These teleological laws have their place in science. They are not, however, universal and general principles. They are, when they are used legitimately, statistical predictions. Advance in science will come from patient observation and experiment which will make clear the circumstances under which these ends will be attained and the proportion of cases in which they will follow certain antecedent conditions, the limits within which these antecedent conditions can vary and still be followed by the interesting outcome. This is the method of Humphrey and of Tolman. It is the method of what Haldane has called the New Physiology. In physiology it has had the admirable results described in Cannon's *Wisdom of the Body*. In the field of psychology it remains an ambition. The psychoanalytic movement has used such a method but without regard for the precautions that are essential to scientific investigation. Their goals are described in such vague and metaphorical terms that whether or not they have been achieved is a matter of subjective opinion. The circumstances under which these goals are to be attained are also inexactly described. And psychoanalysis is almost entirely innocent of statistical methods which are essential to any conclusions in a field in which there is so little control of circumstance. The stray and adventitious antecedent is taken for the necessary and sufficient condition.

The psychoanalysts' difficulties in adopting a scientific method derive from the fact that they are dealing with individuals as individuals. Only when the phenomena they describe can be classified without ambiguity and

many cases be observed with comparable records will they be able to distinguish which among the antecedents of a neurosis will serve to predict the neurosis, and which among the antecedents of the cure will serve to predict the cure. The unique event has all that has recently happened for its antecedents. Only after it has recurred many times can observation discover those signs whose presence serves to warn that the event will follow and whose absence, that the event will default. Before we can blame neuroses on childhood traumas we must do more than show that our neurotics have had such an experience in childhood; we must show that such experiences are absent in normal individuals who fail to develop neuroses. And we must define both neurosis and trauma so clearly that we can agree when they are present and when they are absent.

Eventually we may have a body of psychological observation from which we can draw useful information concerning what to expect from our neighbors, what the results of their learning tend to be, what they tend to learn. At present we have on our list of goals first those very general tendencies which apply alike to the whole species, if not to all the higher animals. This information is taken for granted. It is of startlingly little use. For the teacher to know that her pupils will, whether or not she interferes, tend to maintain themselves as intact organisms does not offer any suggestions concerning how they may be taught to read. She may even be moved to trust so little to this self-preservative tendency which is the law of all living creatures that she will take precautions that

her charges form certain very specific habits in crossing the traffic when they leave for home. In addition to these general tendencies toward self-preservation, food, temperature maintenance, etc., we can by observation of individuals note that they have their individual and specific goal tendencies. One collects stamps; another teases his schoolmates; a third is intent on pleasing the teacher. We may classify their successes in these directions as cases of insight, or we may "explain" their improvement in the pursuit of such goals as instances of maturation, but our satisfaction with such explanations breaks down when we are confronted with instances in which improvement does not take place, or when we desire to interfere and hasten improvement, or when it becomes desirable to do away with a goal-tendency or establish another. In such cases *we must know the circumstances under which improvement fails and the circumstances under which it may be hastened. We must know the circumstances under which interests and desires are established or fail to be established.* We cannot rest contented with statements that life is self-conservative or that improvement in the direction of least action and highest efficiency is a law of psychology as well as physics.

So far as I am aware the only suggestions toward the description or the explanation of the circumstances under which specific changes in behavior will or will not occur have been made in the form of association or conditioning.

The teleological laws which hold true of human be-

havior describe certain end effects which are predictable (with an error, of course) and predictable without regard to the means by which the effects are brought about and without regard to the special circumstances in which the animal is placed. We can say of all organisms that they will somehow continue alive until they die, which they will do only once. This last event will be quite unforeseen if we rely too consistently on the law of self-preservation. Teleological laws include the capacities and abilities of the species. In human psychology they make up what is known as human nature. They include all that we can expect of John Doe from the sole information that he is a man.

A system of psychology which limits itself to such teleological laws neglects the most obvious source of information about the behavior of persons, their past histories. When the past of the individual is used for predicting his behavior we find that our predictions are always in terms of associative learning. If our caller has a record with the police including a number of house-door robberies we assume that there is a strong likelihood that habit, in addition to the general tendency to systemic adjustment, will be again in evidence.

The association-psychology has been charged with leaving John Doe out of the picture. There is a certain truth in the charge. John Doe's quintessence, that something which constitutes his John-Doe-ness, will never be captured in any scientific account. There can be no science of the unique. We can never completely understand

him but we can understand something of him and know something of what to expect of him in terms of what we know of human nature in general and in terms of what we know of his own past history and the nature of associative learning.

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INDEX